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AN ASYMMETRIC APPROACH TO MULTIPLE-FACTOR ANALYSIS

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After a brief history of asymmetric approaches in factor analysis and related fields, there is derived a new asymmetric procedure that avoids communality estimation and other difficulties. This new solution splits a test battery 1 into sub-batteries x and y, each of which spans the same factor space as does a test battery 2. This permits finding the unknown transformation that converts any factorization of the between-battery correlations R_{12} into an orthogonal factor matrix reproducing both R_{12} and R_{xy}, the between-sub-battery correlations.

I. Introduction

The symmetry of the correlation matrix used in factor analysis is something for which we are often justly grateful; and there are well-known advantages in the symmetry of other matrices with which we deal both in that and in other related procedures. On the other hand, recent developments [1, 4] in the field of latent structure analysis have shown that an important gain may be secured by the deliberate *asymmetric* treatment of symmetric matrices, or rather from the concentration on asymmetric sections. The gain consists in the avoidance of certain elements with repeated subscripts that occur in the complete symmetric matrices, and this has a close bearing on the communality problem of factor analysis.

Already there are asymmetric approaches in factor analysis whose principal advantage is that of avoiding the need for communality estimation. One of these [5] is a solution for the proportional profiles problem introduced by Cattell [2, 3]; another the inter-battery method of Tucker [9]. The present paper will derive yet a further asymmetric procedure that is most closely related to that of Tucker. This paper indeed stems from an effort to overcome a weakness in Tucker's procedure that has been discussed in detail elsewhere [6, 7].

In the latent structure solutions just mentioned two symmetric empirical matrices are used—a matrix of pairwise joint frequencies and a matrix of third- or higher-order joint frequencies. Corresponding asymmetric subsections are drawn from them for the solution. The same thing is true for the asymmetric solution of the proportional profiles problem, where the two symmetric matrices from which asymmetric subsections are drawn are correlation matrices for the same variables but based on two different samples differentially selected on orthogonal factors. The Tucker approach strives to get along with but one

[1] Opinions expressed are the author's, not those of the U.S. government.

empirical matrix and one asymmetric subsection therefrom. The result is that, in general, the solution does not give orthogonal factor matrices [6, 7]. A good way of summarizing the asymmetric approach to be derived here is to say that it attempts to make up for the use of but one empirical matrix by using two related but non-overlapping subsections therefrom.

II. Basic Development

It will be instructive to relate the formal development to the analysis of the fictitious example shown in Table I. This same example appeared elsewhere [6], with a correction on its analysis there being made in [7]. In Table I are shown correlations R among eight fictitious tests grouped into a battery 1 and a battery 2, with battery 1 further subdivided into sub-batteries x and y. The grouping is such that the sub-matrix R_{xy}, showing correlations between sub-batteries x and y, has the same rank and refers to the same factor space as does R_{12}, the sub-matrix of correlations between batteries 1 and 2. R_{xy} and R_{12} are blocked in and labelled in Table I. It is the requirement that R_{xy} and R_{12} must imply the very same factor space that is crucial here. Tucker's inter-battery approach uses only an R_{12}. An orthogonal factor matrix F_0 that completely reproduces R is shown at the right in Table I.

It is appropriate to introduce now the general topic of factorization of asymmetric (possibly non-square) matrices such as R_{12}. Here all that is meant by a factoring of an asymmetric matrix R_{12}, of rank r, is the finding of a pair of r-columned and rank r matrices A_1 and A_2, of proper length, such that

$$R_{12} = A_1 A_2'. \tag{1}$$

For the present fictitious R_{12}, where $r = 2$, a proper A_1 is simply the first two columns of R_{12}. Such an A_1 is blocked in and labelled in Table I. That A_1 has rank 2, and weighted sums of its columns will reproduce all columns of R_{12}. The necessary weights are, in fact, the various columns of A'_2, obtainable by standard solutions to systems of linear equations. In the present case, A'_2 can be seen by inspection to be simply

$$\begin{bmatrix} 1{\cdot}000 & 0{\cdot}000 & 1{\cdot}000 & 0{\cdot}000 \\ 0{\cdot}000 & 1{\cdot}000 & 0{\cdot}000 & 1{\cdot}000 \end{bmatrix}.$$

Other ways of factoring R_{12} will be discussed later.

Let us now turn to the more customary meaning of factoring and inquire how we might find a single orthogonal factor matrix F such that its various parts would reproduce both R_{xy} and R_{12} according to the fundamental factor theorem ([8], pp. 79-81). That is,

$$R_{xy} = F_x F'_y, \tag{2}$$

and

$$R_{12} = F_1 F'_2. \tag{3}$$

Here the subscripts on the right indicate the portions of F corresponding to the sub-batteries and batteries that are involved. Now the similar form of eqns. (1)

TABLE I. INTERCORRELATIONS ($R=F_0F'_0$) AND ORTHOGONAL FACTOR MATRIX (F_0) FOR A FICTITIOUS TWO-FACTOR EXAMPLE

			$R=F_0F'_0$									F_0	
			Battery 1				Battery 2						
			Sub-Battery x		Sub-Battery y								
			1	3	2	4	5	7	6	8		A	B
Battery 1	Sub-Battery x	1	1·000	0·000	1·000	0·000	0·566	−0·707	0·566	−0·707	1	1·000	0·000
		3	0·000	0·640	0·000	0·640	0·453	0·566	0·453	0·566	3	0·000	0·800
					R_{xy}								
	Sub-Battery y	2	1·000	0·000	1·000	0·000	0·566	−0·707	0·566	−0·707	2	1·000	0·000
		4	0·000	0·640	0·000	0·640	0·453	0·566	0·453	0·566	4	0·000	0·800
							A_1		R_{12}				
Battery 2		5	0·566	0·453	0·566	0·453	0·640	0·000	0·640	0·000	5	0·566	0·566
		7	−0·707	0·566	−0·707	0·566	0·000	1·000	0·000	1·000	7	−0·707	0·707
		6	0·566	0·453	0·566	0·453	0·640	0·000	0·640	0·000	6	0·566	0·566
		8	−0·707	0·566	−0·707	0·566	0·000	1·000	0·000	1·000	8	−0·707	0·707

and (3) implies the existence of a non-singular linear transformation T such that

$$A_1 T = F_1 \tag{4}$$

and

$$T^{-1} A'_2 = F'_2, \tag{5}$$

for substitution of eqns. (4) and (5) into eqn. (3) yields eqn. (1).

Thus the problem becomes one of finding the transformation T that converts the A_1 and A_2 satisfying eqn. (1) into the orthogonal factor matrices F_1 and F_2. For this purpose eqn. (4) may be split into two parts corresponding to sub-batteries x and y. Thus,

$$A_x T = F_x \tag{6}$$

and

$$A_y T = F_y. \tag{7}$$

Here A_x and A_y are the two parts of A_1 that are associated with sub-batteries x and y, respectively. Now substitution of eqns. (6) and (7) into eqn. (2) yields

$$R_{xy} = A_x T T' A'_y, \tag{8}$$

from which the least-squares expression for TT' is obtainable as

$$TT' = (A'_x A_x)^{-1} A'_x R_{xy} A_y (A'_y A_y)^{-1}. \tag{9}$$

The matrix TT' for the present example, computed according to eqn. (9), is shown at the top left in Table II. Below it is shown a triangular factorization, T, of TT', obtained by what is known, for other reasons, as diagonal factoring ([8], pp. 101–105). To the right of TT' and T in Table II is shown the matrix F_1, computed according to eqn. (4). Beside F_1 is shown F_2, computed by least-squares from eqn. (3) according to the formula

$$F'_2 = (F'_1 F_1)^{-1} F'_1 R_{12}. \tag{10}$$

F_1 and F_2 make up the orthogonal factor matrix F that we are seeking. Comparison of this F and the F_0 of Table I, or of their graphs, indicates that F is essentially a 45 degree rotation from F_0. Reproduction of R by FF' is shown in condensed form at the right in Table II. Because of redundancies in R and F, this 4×4 residual table contains all of the essential information regarding the reproduction of R by FF'. These residuals make clear that the present asymmetric approach has completely recovered, with but rounding error, the known factor space, and without any recourse to the diagonals of R. The end-result of Tucker's recommended factoring procedure ([9], pp. 118–119), using the same R_{12}, would have failed to do so (cf. [7]).

Although derived in connection with a fictitious example having especially simple features (such as the diagonal form of R_{xy}, TT', and T), the foregoing theory and equations are nevertheless quite general and applicable to any set of empirical data having three identifiable sets of vectors (sub-batteries x and y and battery 2) spanning the same r-dimensional factor space. Thus the order of R must be at least three times as great as its rank. If it were less, then at

TABLE II. ASYMMETRIC SOLUTION AND TEST OF FIT FOR A FICTITIOUS TWO-FACTOR EXAMPLE

TT'			$F_1=A_1T$		$F_2=R'_{12}F_1(F'_1F_1)^{-1}$			$R-FF'$ (Condensed)			
5	7		I	II	I	II		1	3	5	7
5	1·556	0·000	1	0·706	−0·707	5	0·802	0·000	1	−0·002			
7	0·000	1·000	3	0·565	0·566	7	0·001	1·001	3	0·001	0·000		
			2	0·706	−0·707	6	0·802	0·000	5	0·000	0·000	−0·003	
			4	0·565	0·566	8	0·001	1·001	7	0·000	−0·001	−0·001	−0·200

T		
	I	II
5	1·247	0·000
7	0·000	1·000

least some of the diagonals of R would have to appear in R_{xy} or R_{12}, and presumably one of the principal advantages of an asymmetric approach is the avoidance of those diagonals (cf. [9], abstract on pp. 111, 113, 135).

III. Factoring Methods

In empirical applications there will of course be problems that did not arise in our fictitious example. The first of these is to determine the rank of R (or of some particular subsection R_{12} that may be of special interest). In effect, this is the question of finding an R_{12} of maximum rank (or of determining the rank of a particular R_{12} if it is of special interest). Some trial-and-error, in which various R_{12}'s are tried, may be unavoidable here, but for a particular empirical R_{12} the problem of determining its rank is reducible to that of fitting it with a factorization of minimum rank.

The simple factoring procedure that was used here, in which a set of r linearly independent columns of R_{12} was used as an A_1, might be useful in preliminary explorations as to the possible rank of a particular R_{12}, and especially as to a lower bound thereof. That procedure could be adapted to extract one dimension at a time, for taking some one column of R_{12} as the first column, $A_{1\text{I}}$, of A_1, the first row, $A'_{2\text{I}}$, of A'_2 could be quickly found by least-squares. That is, given an $A_{1\text{I}}$, the associated post-multiplier $A'_{2\text{I}}$ satisfying

$$R_{12} = A_{1\text{I}} A'_{2\text{I}} \tag{11}$$

with a minimum sum of squared discrepancies is given by

$$A'_{2\text{I}} = (A'_{1\text{I}} A_{1\text{I}})^{-1} A'_{1\text{I}} R_{12}. \tag{12}$$

Here, of course, the matrix $(A'_{1\text{I}} A_{1\text{I}})^{-1}$ is simply the reciprocal of the sum of squared entries in $A_{1\text{I}}$, and the matrix $A'_{1\text{I}} R_{12}$ is just a row vector whose elements are weighted sums of the rows of R_{12}. Those weights are the elements of $A_{1\text{I}}$.

Given an $A_{1\text{I}}$ and its associated matrix $A'_{2\text{I}}$, the residual matrix

$$R_{12\cdot\text{I}} = R_{12} - A_{1\text{I}} A'_{2\text{I}} \tag{13}$$

could be formed, a column of it taken as the second column, $A_{1\text{II}}$, of A_1, and the associated least-squares $A_{2\text{II}}$ computed by

$$A'_{2\text{II}} = (A'_{1\text{II}} A_{1\text{II}})^{-1} A'_{1\text{II}} R_{12\cdot\text{I}}, \tag{14}$$

etc. This process could be continued until the non-vanishing residuals were sufficiently small and randomly arranged to be considered as unimportant errors. At that point the number of columns in A_1 or rows in A'_2 would be the working rank for that R_{12}.

The factoring method just outlined is analogous to the diagonal method of factoring ([8], pp. 101–105) in that it provides perfect fit for those columns of R_{12} and subsequent residual tables from which the columns of A_1 are taken, but at the expense of poorer fit for the remaining columns of R_{12}. This defect could be removed by using row sums of R_{12} and subsequent residual tables, rather than single columns, as columns of A_1. This would make it resemble

centroid factoring ([8], pp. 160–170), and, as in centroid factoring, there would be the possibility of reflecting columns of R_{12} and subsequent residual tables. The purpose of such reflections would be to maximize the absolute values of the row sums used as columns of A_1. A slight improvement here might be to use row averages rather than row sums for A_1, in order that the magnitudes in the corresponding A'_2 might be commensurate with those in A_1.

The foregoing ' diagonal ' and ' centroid ' factoring methods resemble their namesakes only in the formation of A_1. The A'_2 is, in each case, the least-squares ' partner ' to the corresponding A_1, for each row thereof, given the corresponding column of A_1, is such as to minimize the sum of squares of the remaining residuals. Thus A'_2 resembles a principal axis factor matrix in its fitting properties ([8], p. 482). It is then natural to ask whether it is possible to have least-squares properties in both A_1 and A'_2. Suppose one started with an arbitrary first column for A_1 (call it A_{1I0}) and obtained the associated A'_{2I0} from eqn. (11) by least-squares as

$$A'_{2I0} = (A'_{1I0}A_{1I0})^{-1}A'_{1I0}R_{12}. \tag{15}$$

Now, using the A'_{2I0} computed by eqn. (15), one could compute *its* least-squares partner, A_{1I1}, from eqn. (11) as

$$A_{1I1} = R_{12}A_{2I0}(A'_{2I0}A_{2I0})^{-1}. \tag{16}$$

But substitution of eqn. (15) three times into eqn. (16) gives

$$\begin{aligned} A_{1I1} &= R_{12}R'_{12}A_{1I0}(A'_{1I0}A_{1I0})^{-1}[(A'_{1I0}A_{1I0})^{-1}A'_{1I0}R_{12}R'_{12}A_{1I0}(A'_{1I0}A_{1I0})^{-1}]^{-1} \\ &= R_{12}R'_{12}A_{1I0}(A'_{1I0}A_{1I0})^{-1}(A'_{1I0}A_{1I0})(A'_{1I0}R_{12}R'_{12}A_{1I0})^{-1}(A'_{1I0}A_{1I0}) \\ &= R_{12}R'_{12}A_{1I0}(A'_{1I0}R_{12}R'_{12}A_{1I0})^{-1}(A'_{1I0}A_{1I0}). \end{aligned} \tag{17}$$

Now eqn.(17) can be related to something familiar on two counts. First, the expression $R_{12}R'_{12}$ in it is the symmetric matrix

$$H_1 = R_{12}R'_{12} \tag{18}$$

whose principal axis factorization provides the A_1 of Tucker's inter-battery paper ([9], pp. 118–119). Second, the *form* of eqn. (17) is suggestive of the starting point for Hotelling's iterative method (cf. [8], p. 483) for obtaining the principal axis factors of H_1. To see this more clearly, note that eqn. (17) could be written as

$$A_{1I1} = H_1A_{1I0}k, \tag{19}$$

where the whole expression

$$k = (A'_{1I0}R_{12}R'_{12}A_{1I0})^{-1}(A'_{1I0}A_{1I0}) \tag{20}$$

is but a constant or scalar multiplier. Equation (19) shows that, except for an unimportant constant multiplier, A_{1I1} is obtained simply by post-multiplying H_1 by A_{1I0}. But if this process is continued (i.e., if H_1 is post-multiplied by A_{1I1} to get an A_{1I2}, and then by A_{1I2} to get an A_{1I3}, etc.), it will converge to a point where two successive A_{1I}'s are proportional to each other and to the first principal axis factor loadings of H_1 (cf. [8], p. 483). In the present context, this continuation amounts to finding successive least-squares counterparts,

i.e., A'_{2I1} for A_{1I1}, then A_{1I2} for A'_{2I1}, etc., but with no A'_{2I} ever appearing explicitly. Arrival at convergence signifies that no further improvement is possible, whereupon the resulting A_{1I} and A'_{2I} take on the interesting property that each is least-squares counterpart to the other.

The foregoing reasoning can be extended to subsequent columns of A_1 and rows of A'_2, though the details will not be undertaken here. The essence of the extension lies in the fact that if a complete A_1 whose columns are proportional to the principal axis factor loadings of H_1 is substituted for A_{1I0} in eqn. (17), the expression denoted k in eqn. (20) becomes a diagonal matrix. Thus the principal axis factors of H_1 provide least-squares fit, not only for H_1, but also for R_{12}.

Another way of seeing this is to paraphrase a development of Thurstone ([8], p. 482) and write the expression for the general first-factor residual, ρ_{jk}, for the general entry, r_{jk}, of R_{12}, thus:

$$\rho_{jk} = r_{jk} - a_j a_k. \tag{21}$$

Here a_j and a_k are first-factor loadings taken from A_1 and A'_2, respectively, and it should be understood in what follows that all a_j's are from A_{1I} and all a_k's are from A'_{2I}. Squaring the residual in eqn. (21) gives

$$\rho^2_{jk} = r^2_{jk} - 2a_j a_k r_{jk} + a^2_j a^2_k, \tag{22}$$

and summing over all residuals yields

$$\Phi = \sum_j \sum_k \rho^2_{jk} = \sum_j \sum_k r^2_{jk} - 2\sum_j \sum_k a_j a_k r_{jk} + \sum_j \sum_k a^2_j a_k{}^2. \tag{23}$$

To minimize Φ we set equal to zero its partial derivatives with respect to a particular a_j and a particular a_k, getting

$$\frac{\partial \Phi}{\partial a_j} = -2\sum_k a_k r_{jk} + 2a_j \sum_k a^2_k = 0 \tag{24}$$

and

$$\frac{\partial \Phi}{\partial a_k} = -2\sum a_j r_{jk} + 2a_k \sum_j a^2_j = 0, \tag{25}$$

or

$$\sum_k a_k r_{jk} = a_j s_2 \tag{26}$$

and

$$\sum_j a_j r_{jk} = a_k s_1, \tag{27}$$

where s_2 and s_1 are, respectively, the sums of squared entries in A'_{2I} and A_{1I}. Now when eqns. (26) and (27) are written out for all values of j and k, the results can be stated in the matrix equations

$$R_{12} A_{2I} = A_{1I} s_2 \tag{28}$$

and

$$R'_{12} A_{1I} = A_{2I} s_1. \tag{29}$$

Equation (29) can be solved for A_{2I} to be substituted into eqn. (28), with the result, after multiplying through by s_1, that

$$R_{12}R'_{12}A_{1I} = A_{1I}s_1s_2. \tag{30}$$

But this equation merely states the unique convergence or self-generative property (cf. [8], p. 483, and earlier discussion here) of a column vector A_{1I} that is proportional to the first principal axis factor loadings of $R_{12}R'_{12}$, or H_1. Thus again we arrive at the result that a least-squares A_{1I} is obtainable from the principal axis factorization of H_1, and the same thing could be shown for subsequent columns of A_1.

IV. Other Problems

Once a particular R_{12} has been made to yield, by some factoring procedure, an A_1 of rank r, there remains the question whether that A_1 can be divided into *two* parts, A_x and A_y, *each* of which is of rank r. The various R_{12}'s should be formed with some foresight for this problem. Here a guide is the requirement that R_{xy} must have rank r. Even so, some amount of trial-and-error on this score might be unavoidable. For this reason, as well as for the purpose of investigating the overall rank of a number of possible R_{12}'s, it might be less costly computationally to factor them initially by a less expensive procedure such as the ' centroid ' method outlined earlier, and to factor only the final R_{12} by the complete least-squares approach. This of course depends upon available computing facilities.

When least-squares factoring is to be applied, some computational savings may be realized by finding A_2 rather than A_1. This is because H_1 must be at least $2r \times 2r$ in size in order that A_1, once obtained, may be divided into the two sections A_x and A_y, each having at least r rows. The least-squares A_2, on the other hand, is obtainable quite analogously from the principal axis factorization of

$$H_2 = R'_{12}R_{12} \tag{31}$$

([9], pp. 116–117), which may be as small as $r \times r$ if of full rank. Thus in many empirical applications both the formation and the least-squares factorization of H_2 may involve less numerical work than for H_1. Given A_2, A_1 is readily obtainable by least-squares from eqn. (1) as

$$A_1 = R_{12}A_2(A'_2A_2)^{-1}, \tag{32}$$

for here the inverse is diagonal because A_2 is a principal axis factorization (cf. [8], p. 501).

Once the A_x, A_y, and R_{xy} are settled upon, there remains only their use in eqn. (9) to obtain TT'. Formation of A'_xA_x and A'_yA_y and their inversion will test the rank of A_x and A_y, for if either product is not of rank r, its inverse will (as it is popularly put) 'blow up'. Thus large entries in either of the computed inverses constitute a warning as to weak rank r. Conceivably this weak rank could arise merely from scaling down some columns of A_1 to a point

where their contribution to the products A'_xA_x and A'_yA_y is overpowered by other columns. This should be guarded against in advance.

A further problem with empirical data arises from the fact that, although the computed TT' is theoretically a symmetric matrix, it will seldom be exactly so in practice. The expression on the right in eqn. (9) is not symmetric in form. Severe departures from symmetry indicate important violations of the theoretical requirement that R_{xy} refer to the same factor space as does R_{12}. Even when R_{xy} and R_{12} have the same rank r, and hence the same *number* of factors, the *nature* of the factors could differ in various ways. A still more insidious possibility is that, though the two sets of factors might differ in nature, they could conceivably interact so as to allow the computed TT' to be fairly symmetric: in other words, symmetry of the computed TT' is a necessary but not a sufficient condition for applicability of the present inter-battery approach. How precisely distortions such as this might arise is hard to see; but usually they should manifest themselves in a poor reproduction of R_{xy} by the appropriate parts of the resulting F.

Minor deviations from perfect symmetry of the computed TT' are probably unavoidable in empirical applications, so that a 'symmetrizing' procedure (to use a word suggested by L. L. Thurstone) is necessary. The simplest possibility would be to add the computed TT' to its transpose and divide all entries in the resulting matrix sum by two. That would amount to treating the jk and kj elements in the computed TT' as equally fallible estimates of the same quantity, and taking their average.

No special problems should arise in factoring the symmetrized TT' in an empirical study, for r real 'factors' are to be extracted from an $r \times r$ matrix, so that there is no fitting problem. Diagonal factoring should be as good as any in this context, unless some other method would fit in better with subsequent rotational objectives. If the last one or more factors extracted are very weak, and no difficulties were encountered in inverting A'_xA_x or A'_yA_y, it may be a sign that the R_{xy} being used has weak rank r. If it should happen that R_{xy} has rank greater than r, that would create no difficulty in factoring, but it would constitute a violation of the requirement that R_{xy} and R_{12} must refer to the same factor space, so that the resulting fit for R_{xy} would be poor.

Once a transformation T is found, its application as a post-multiplier on A_1 to get F_1 by eqn. (4) is straighforward, whereupon F_2 is obtainable by eqn. (10) or eqn. (5). Equation (5) may be preferable to eqn. (10) for obtaining F_2 if A_2 has already been obtained explicitly, and if T is readily inverted, as it would be if triangular or if a principal axis factorization. If T is triangular, the elements of its inverse (also triangular) can be obtained one at a time using the definition $TT^{-1} \equiv \mathrm{I}$. If it is a principal axis factorization of the symmetrized TT', then it is proportional by columns to an orthogonal transformation ([8], p. 501), and its inverse is simply its transpose with each row divided through by the respective sum of squared elements.

Forming F_1 and F_2 into the complete F is the last step in the present asymmetric factor approach. Tests of fit of any part of R by the appropriate parts of F are now possible, and any type of rotational adjustment that is desired may be applied to F.

V. Conclusions

This paper has briefly outlined the history of asymmetric solutions in latent structure analysis and factor analysis, and has presented a new asymmetric solution for factor analysis, together with suggestions on how to overcome problems that arise in dealing with empirical data. It is believed that this asymmetric approach will prove useful in practical application, more especially where the investigator desires to avoid the estimation of communalities.

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A NOTE ON ROTATION AND PSYCHOLOGICAL SPACE[1]

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"You may clothe a vector space in an inner product; but it's born naked."

The purpose of this note is threefold: to consider the conditions under which the concept of distance (both physical and psychological) and angle (the pure number function of distance) are meaningful; to suggest that there is some evidence that these conditions do not obtain throughout psychological space; and to consider briefly the implications of this evidence. My argument will be that distance (and therefore angle) is meaningful only when the space involved possesses the property of dimensional isotropy.

I. Isotropic Space

As I shall use the term, isotropic space is one in which, in principle, measurement operations can be identical no matter what direction one wishes to measure. The long-standing use of standard scores in psychology seems to have misled many psychologists into the uncritical and unrecognized assumption of the isotropy of most psychological spaces[2]. And the question I am raising is whether or not the *dimensions* of various psychological spaces are such that these spaces *can* be isotropic. Consider, for instance, the two-dimensional space defined by the measurements height (cm) and weight (kg) of a sample of adult males. If these pairs of scores be plotted, all psychologists will expect the bivariate scattergram to be elliptical, the major axis being inclined to the horizontal. It is easy to see that, relative to the original reference-variables (height and weight), this major axis has an equation of the form: $a \,.\, x$ cm $+$ $b \,.\, y$ kg $= K$ (units unspecifiable). Now suppose someone were to propose that we project each of our points perpendicularly on to this major axis, and, having selected two arbitrary points on that line determining an origin and a unit, that we measure each projected point on this line in terms of that unit. This new dimension will be the one along which we should measure if we wished, by means of a single measurement, to maximize the information concerning individual differences contained in the original set of *pairs* of measurements. Yet no one would consider 'distances' along this 'dimension'

[1] I am much indebted to Dr. John P. McKee for assistance with the argument and its exposition.

[2] One way of avoiding the difficulties inherent in the use of distance notions is to refuse all recognition to psychological variables. This solution is that of the consistent behaviourist, who is concerned solely with the relations between the input and output events on the two sides of the organism—'a psychology', in Egon Brunswik's terms, 'without the organism'. But the continued vitality of the factor analytic approach, as well as the studies of stimulus generality and of the dimensions of similarity, suggest that not all the problems of interest to psychologists will yield to the former type of approach.

meaningful or even practically useful. The reason is that whether or not he has given any consideration to the physicist's *dimension analysis*, it is perfectly obvious that you can not meaningfully add heights to weights. Moreover it is equally clear that were we to convert height and weight to standard scores the corresponding major axis transform is still meaningless, and there is not the slightest possibility of ever being able, independently of measures of height and weight, to determine a ' score ' in this ' dimension '.

Yet factor analysts seem to believe that for any two psychological tests (I set aside the whole problem of scaling) just such major axis transformations are perfectly legitimate and useful so long as the original scores are first converted to standard scores. This case is as fallacious as the former, and for the same reason.

Consider a hypothetical example. An investigator obtains (i) a measure of religious devoutness, (ii) a measure of the difference limen for monochromatic light of 490 millimicrons, and (iii) a measure of oral fixation. He then plots each of his subjects in the resultant three-dimensional ' space '. In general, so long as he sticks to his original data and so long as he does not measure more than three variables, he is not tempted to rotate his axes because he is immediately struck with the nonsense of a metric that would consist in some combination of the three sets of measuring operations. But two things conspire to mislead the factor analyst. First, he always begins with more than three dimensions, and so can not plot his data. Secondly, he not only converts his data to standard scores, but he then proceeds to correlate his dimensions: that is, he fixes his attention on a matrix formed by the product of the one we have described by its own transpose. In carrying out this arithmetical ritual, he completely loses sight of his original operational dimensions.

In ordinary space, all three dimensions are of the same kind—distance, as measured by yardsticks, calipers, and so on. Though there are many different practical operations for measuring this physical distance, it is possible, in principle, to use only one, and to use that one for each of the three spatial dimensions commonly called length, breadth, and depth. Further, it is possible to use this single operation to measure this same physical distance between points located anywhere in the space: that is, *distance throughout the space has the same meaning even when it is measured in directions which are oblique to any or all of the arbitrary reference axes.* All kitty-corner distances have the same meaning as distances parallel to the axes.

II. Three Senses of the Term Dimension

To make clear what I mean by *dimensional homogeneity* or isotropy, it is necessary to distinguish between three important senses in which the term ' dimension ' is used. One use is simply to indicate the operations determining one member of a set of coordinates which specifies a referent in its reference

space. In this sense, height is an operational dimension; as applied to persons, so are religious devoutness, difference in threshold for a particular wavelength of light, and oral fixation. The chief operational dimensions used in the description of physical objects include length, width, height, weight, volume, temperature, latitude, pressure, and electrical resistance.

One of the assumptions of some brands of factor analysis is that the number of psychological dimensions commonly used to describe the reference objects (i.e. the persons measured) is larger than is necessary for the complete specification of those objects. Hence, applying Occam's razor, we start a search for the minimal number of common reference axes.

This brings us to the second use of the term ' dimension '—to refer to a member of such a *minimal set* of axes. The factor analytic search for a minimal set of psychological dimensions is based on an analogical argument from physical space. Suppose, for instance, that each of a number of points on a line (or on a plane) were to be specified by a set of three coordinates and that the line (or plane) in question did not coincide with one of the reference axes (or with a plane determined by a pair of them). In this case some of the original dimensions used are unnecessary, since an appropriate selection of a smaller set of dimensions would suffice to specify each object uniquely and to describe all the relations among them. For example, each point on the line could be uniquely specified by its distance from an arbitrary origin. Similarly, in the plane, an arbitrary origin and any two reference axes would permit a unique specification of each point in it.

Let me take a further example—specifying the point at the lower southeast corner of the southernmost eave-support of my house. A minimum of three spatial coordinates will be required. The three spatial dimensions are specified each by a distance; and the space defined by these three dimensions is dimensionally isotropic because in principle the two distances from the property lines and the height above the foundation can be measured in the same way.

In this space we can transform the coordinate specification of a point from one system of coordinates into a variety of other systems. So, for example, we learn of rectangular and oblique Cartesian coordinate systems, as well as polar coordinate systems in the two-dimensional space of the plane, and other coordinate systems for the spatial aspects of the ' real world ' could readily be devised if required. Finally, in this isotropic space we may rotate our reference axes to any convenient arbitrary position, because the measurement operations in the various directions are identical. Our familiarity with these facts of everyday space must share responsibility with our use of standard scores for our failure to recognize the fallacies of assuming isotropy where it does not obtain.

In dimensionally non-isotropic space we can use only a limited class of transformations. We may displace the origin, and we may alter the size of the units in terms of which one or more of the reference magnitudes are expressed. But rotational transformations are meaningless because in such a space no

meaning whatever attaches to a generalized distance. For differences between points in a non-isotropic space there is no alternative except to use the names of the particular operational dimensions which are involved, whether those dimensions be physical (e.g. duration, mass, length) or psychological (e.g. religious devoutness, oral fixation, difference limen). No generalized metric can be associated with differences in location: we must be content with vectors of differences.

The general class of transformations which we learn in analytic geometry is appropriate in its entirety only to a very special type of space—the dimensionally isotropic space. The notions of generalized distance and angle (the pure-number function of such generalized distance) are appropriate *only* to such a space. Unfortunately ordinary everyday ' space ' is of this special kind; though multi-directional, it is only uni-dimensional.

I turn now to the third sense in which the term ' dimension ' may be used—one that is extremely important. When the layman speaks of time-space as four-dimensional, he means that he needs three spatial coordinates and one temporal coordinate. But there is a real sense in which this time-space is only two-dimensional: viz., the sense that only two fundamentally different kinds of operation are required to determine the four coordinates specifying the system in question. These are the operations of measuring distance and those of measuring time.

It may be objected that this time-space is really three-dimensional inasmuch as any world-point—to use Minkowski's (1913) phrase—may equally well be identified by two angles, one length, and one time: three fundamentally different kinds of operation are involved in measuring distance, angle, and duration. My reply is that it is possible to measure angles in terms of lengths, so that we can analyse the measurement operations into only two fundamentally different kinds. It is this minimal dimensionality which is sought by those varieties of factor analysis which seek unique solutions to the rotation problem.

A second objection is that the two-dimensional time-space may in fact be only one-dimensional: we have not as yet been able to make the proper analysis. Minkowski's demonstration that the Lorentz transformations (introduced to account for difficulties in which physics was involved by the failure to detect the ether-drift expected from Newtonian mechanics) represented simply a rotation of reference-axes in a four-dimensional space seems to suggest precisely such dimensional homogeneity. Minkowski's equations " represented the rotations of the coordinate system in a four-dimensional continuum (space and time combined) with an indefinite metric " (Bergman 1956). Minkowski certainly suggests the dimensional homogeneity of the space-time continuum, though his paper does not make clear just what operations could be used to measure distance along the various space-like (' raumartiger ') or time-like (' zeitartiger ') directions within space-time.

III. The Criterion for Isotropy

The absence of specifiable operations for scaling should not obscure one important point, namely, that *all physical laws which are stated in terms of the space-time manifold of relativity assume the same form, irrespective of the type of Minkowski transformation of the coordinate system that has been used.* It is this which makes the space-time continuum dimensionally homogeneous or isotropic.

Psychology has two alternatives. The first is somehow to demonstrate that such *operationally different* dimensions as religious devoutness, visual difference limen, and oral fixation (or Binet IQ, ascendance-submission, ethnic prejudice and socio-economic status), etc., can nevertheless in principle be measured by the same operation[1]. 'Spaces' defined by such operationally equivalent dimensions would presumably be dimensionally isotropic, and rotation of reference axes within such spaces would correspond to psychologically meaningful operations. The other is to establish laws involving various operational dimensions, and then to determine empirically whether the mathematical forms of those laws remain constant when the reference axes are rotated in whatever tentative 'space' is selected. If they do, the space is isotropic; otherwise, not. In the former case, the notion of psychological distance will be valid. In the latter it will be meaningless, since there are no procedures for demonstrating differences or equalities between such distances.

Attneave (1950), in an interesting paper, has made a preliminary examination of the space defined by judgments of visual differences and similarities, and indeed has anticipated some aspects of my discussion. In considering the hypothesis that psychological space is Euclidean, he remarks that the " most important psychological implication is that, within a space of given dimensionality, one set of axes or one frame of reference is as good as any other. For example, colours are commonly described in terms of brightness and saturation. If psychological space were Euclidean, it would be entirely possible and psychologically proper to rotate these axes and discuss colour in terms of attributes which might be termed 'sightness' and 'braturation', axes cutting across those we generally use " (p. 521). Attneave's own data led him to doubt the Euclidean hypothesis.

So far, psychological factor analysts have tacitly assumed the isotropy of their psychological space. They have thus cavalierly ignored the difficulty which the physicist has in *demonstrating* his isotropy. The physicist has developed his methods for measuring magnitudes, and establishing quantitative relations among his operational dimensions, on the assumption that time-space involved two different fundamental dimensions. Experimental findings revealed the theoretical usefulness of the assumption of homogeneity. But factor

[1] Cattell's (1946) extensive list of factor analytic studies of personality reveals that in most cases factor analysts have dimly recognized the importance of dimensional homogeneity in selecting tests for analysis. One does not usually find, in any real factor analysis, so heterogeneous a collection of operational dimensions as our illustrative 'religious devoutness', 'difference threshold', and 'oral fixation'.

analysis, deceived by its standard scores and the arithmetical identity of computing standard scores for its heterogeneous dimensions, has presumed out of hand that psychological space, analogous to the three-dimensional space of the real world, is dimensionally homogeneous. I conclude that factor analysts have been premature in assuming (as they do in some defences of rotation) that the many operational dimensions of psychological space involve only one *fundamental* dimension. I am impressed with the criterion—admittedly extralogical—that the form of laws remains invariant under all rotational transformations if dimensional homogeneity obtains; but the psychologist has few quantitative laws to which he can apply this criterion.

It seems likely that the search for rotational criteria by factor analysts is related to the problem of dimension analysis, though I have been unable to find any explicit statement to this effect: (see, however, Attneave 1950). Thurstone has emphasized the notion of simple structure as a criterion for rotation. Anyone who would combine such a criterion with the expectation of a unique solution would be committed to the view that psychological space is not isotropic. Here the dimension-analysis approach suggests three possibilities for psychology. The first is that if a particular psychological ' space ' is isotropic there can be no uniquely correct orientation of the reference axes: convenience alone will decide which orientation shall be used. This was the position taken by Thurstone before he began to emphasize simple structure. The second is that the dimensions of a particular psychological space are *not* in principle measurable by the same operations and that there is no single fundamental dimension which pervades it. In this case, a unique and ' psychologically meaningful ' rotation of the reference axes might be sought, but the concept of the communality as a metric-less distance becomes meaningless. The third possible psychological space is one for which there are different sets of operational dimensions; within each there might be dimensional homogeneity, while across sets there might be none. Spearman, with his general and special factors, seems to have been closest to recognizing this possibly. Though such a space might claim the advantages both of partial isotropy and of psychological meaningfulness, I suspect that the notion of a partial isotropy will prove very elusive. In any event, psychology seems so far from demonstrating that IQ, masculinity, ethnocentrism, creativity, psychopathy, etc., can all be measured in the same way that to seek reference axes for ' spaces ' defined by dimensions that are operationally so different is probably fruitless. As a practical matter the factor analysis of such complex ' spaces ' serves the useful end of reducing a mass of data to manageable proportions, and of making some statistical predictions; but it does no more. Meaningful sets of reference axes are not likely to be found in such spaces.

IV. Conclusion

Psychologists have long been concerned with notions of psychological distance. Psychophysicists and modern experimental psychologists as well as

differential, clinical, and personality psychologists have found reasons for considering this concept. Although not all have explicitly dealt with distance, a certain invariance notion, closely related to the measurement of distance, has often been implicit in their thinking. Sometimes they find it useful to transform the reference axes of their psychological space—particularly those which entail only displacement or uniform shrinking or stretching of the unit reference vectors. But rotational transformations are legitimate only in spaces that are dimensionally isotropic. By this is meant that the space must be of such a character that, in principle, *identical* operations can be used to measure in each of the infinite number of conceivable directions. Whether or not the conceptual spaces employed by the psychologist possess this property is an empirical question.

Experimental work on stimulus generalization and on the dimensions of similarity suggests that some dimensions of sensory space are *not* dimensionally isotropic. The ultimate criterion is the *formal* invariance, under any transformation of the kind mentioned above, of the *laws* relating psychological variables. At present, psychology has few, if any, magnitudes, and hence few if any laws, whose formal invariance could provide evidence for dimensional isotropy.

The factor-analyst's search for convincing rotational criteria reflects concern about the isotropy of common factor space; but his invariance criteria are not appropriate for testing this isotropy. The possibility of such non-isotropy of common factor space suggests that both those who would rotate arbitrarily and those who would rotate to psychological meaningfulness have truth on their side. The arbitrary rotators are correct in so far as their common factor space contains at least one multidimensional subspace which *is* itself isotropic; the others are correct to the extent that common factor space contains at least two such sub-spaces which are not isotropic with each other. The search for simple structure perhaps reflects concern with the problem of identifying several mutually non-isotropic subspaces. But the ideal of a unique solution for every rotational problem indicates a tacit conviction that the minimal set of reference axes necessary to describe a psychological space will contain no two directions which have the same fundamental dimensionality, and, if they do not, then the notion of a generalized psychological distance becomes meaningless.

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THE MEANS OF SECTIONS FROM A NORMAL DISTRIBUTION

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The following paper gives a table for the mean abscissae of vertical sections of the normal curve. The need and usefulness of such a table are illustrated by problems from the field of mental measurement.

I. Problem

In both psychological and educational inquiries the investigator has frequently to deal with a group of individuals who have been rated by categories. In many large-scale examinations, for instance, the assistant examiners are allowed to use a scale of literal marks, and the chief examiner may then desire to reduce them to numerical equivalents on the basis of the proportion or percentage of the candidates who are awarded this or that mark. Usually it is assumed—though the assumption is not necessarily true—that the population so marked is normally distributed in respect of the underlying variable, so that the categories form sections of the total distribution (cf. Burt 1943, Sandon 1952). Or again, the investigator may be confronted with two populations, both normally distributed, with the same standard deviation, but with different means (e.g. a group of boys and a group of girls from a given educational area, who have both taken an examination in which one sex is superior to the other, boys, for instance, in arithmetic, or girls in English); and the same line of demarcation has then been applied to both groups, so that different percentages have been selected. The investigator, knowing only the borderline and the two percentages, may wish to estimate the means for the two selected tails.

Formulae for assessing the means of a vertical section or of a tail selected from a normal distribution are already available in the statistical literature (Kelley 1923). But for practical purposes it would plainly be helpful to have a single table of values, instead of having to make an *ad hoc* calculation on each occasion.

II. The Construction of the Table of Means

One of the unique properties of the normal curve is the relation between the mean deviation of a section of the total distribution and the difference between the two ordinates, upper and lower, which cut off the section. Thus, if z_1 and z_2 are the ordinates, q_1 and q_2 the areas of the tails cut off by these ordinates,

and $\bar{x}$ is the mean deviation of the section

$$\bar{x} = \frac{z_1 - z_2}{q_1 - q_2}.$$

(see Kelley 1923, pp. 100f.)

The meaning and use of the formula may best be understood by the following example suggested to me by Professor Burt for the distribution of intelligence according to occupational class (see this *Journal*, XIV, p. 11, Table I.) In the table below the figures in the first column give the percentages in each social class. From these the values for q, are obtained by cumulative summation, and the values for z_1, from Kelley's table (*loc. cit.*, Appendix C); the values for q_2 and z_2 are the same as those for q_1 and z_1 in the category just preceding. It is assumed that the entire distribution is strictly normal and that the standard deviation is 15 I.Q. points.

Table I. An Illustrative Example

Class	Percentages	q_1	z_1	$\bar{x}$	Mean I.Q.
I	0·3	0·003	0·00915	3·05	145·7
II	3·1	0·034	0·07545	2·14	132·1
III	12·2	0·156	0·23930	1·34	120·1
IV	25·8	0·414	0·38964	0·58	108·7
V	32·5	0·739	0·32501	−0·20	97·0
VI	26·1	1·000	0·00000	−1·24	81·3

Thus for class V we have

$$\frac{z_1 - z_2}{q_1 - q_2} = \frac{0{\cdot}32501 - 0{\cdot}38964}{0{\cdot}739 - 0{\cdot}414} = -0{\cdot}20 \text{ S.D. or } 97{\cdot}0 \text{ I.Q.}$$

The actual value for the mean, calculated from the detailed test measurements, was 97·8 I.Q. In the case of the other classes, particularly those in which the numbers are very small, the discrepancies are rather larger—as after all was only to be expected with a sample that could hardly be expected to conform precisely to a normal distribution.

To use the table given below (p. 119) the figures for the percentages have first to be converted into terms of standard deviations. Indeed, the table itself is primarily designed for those investigations in which the borderlines are given in terms of actual deviations. The estimates in the table have been calculated from the values given by Pearson and Hartley (1954), Table I; (cf. K. Pearson, 1931, Table II). In these tables X corresponds with Kelley's x, $Z(X)$ with Kelley's z, and $P(X)$ with Kelley's q or p $(=1-q)$. In Table II (p. 119) the means are given to four decimal places. In the case of the most extreme values the figures in the last decimal may be slightly inaccurate.

III. Applications of the Table

To illustrate the type of problem described above, let us suppose that, in the child population with which the investigator is concerned, there is a small

TABLE II. MEAN ABSCISSAE OF SECTIONS OF NORMAL CURVE

Lower Boundary	Width of Section (in standard deviations)								
	0·25	0·5	0·75	1·0	1·25	1·5	1·75	2·0	∞
−3·00	−2·8602	−2·6949	−2·5112	−2·3158	−2·1144	−1·9110	−1·7087	−1·5100	+0·0044
−2·75	−2·6114	−2·4496	−2·2710	−2·0816	−1·8867	−1·6904	−1·4959	−1·3058	+0·0091
−2·50	−2·3627	−2·2045	−2·0310	−1·8481	−1·6605	−1·4723	−1·2867	−1·1065	+0·0176
−2·25	−2·1140	−1·9593	−1·7914	−1·6153	−1·4357	−1·2565	−1·0810	−0·9123	+0·0321
−2·00	−1·8653	−1·7143	−1·5520	−1·3832	−1·2122	−1·0430	−0·8788	−0·7228	+0·0552
−1·75	−1·6166	−1·4693	−1·3128	−1·1517	−0·9900	−0·8316	−0·6798	−0·5377	+0·0899
−1·50	−1·3679	−1·2243	−1·0738	−0·9206	−0·7688	−0·6220	−0·4835	−0·3563	+0·1388
−1·25	−1·1192	−0·9794	−0·8350	−0·6901	−0·5485	−0·4138	−0·2892	−0·1775	+0·2042
−1·00	−0·8705	−0·7345	−0·5964	−0·4599	−0·3288	−0·2066	−0·0963	0·0000	+0·2876
−0·75	−0·6218	−0·4897	−0·3577	−0·2299	−0·1098	0·0000	+0·0963	+0·1775	+0·3894
−0·50	−0·3731	−0·2448	−0·1193	0·0000	+0·1098	+0·2066	+0·2892	+0·3563	+0·5092
−0·25	−0·1244	0·0000	+0·1193	+0·2299	+0·3288	+0·4138	+0·4835	+0·5377	+0·6458
0·00	+0·1244	+0·2448	+0·3577	+0·4599	+0·5485	+0·6220	+0·6798	+0·7228	+0·7979
+0·25	+0·3731	+0·4897	+0·5964	+0·6901	+0·7688	+0·8316	+0·8788	+0·9123	+0·9636
+0·50	+0·6218	+0·7345	+0·8350	+0·9206	+0·9900	+1·0430	+1·0810	+1·1065	+1·1411
+0·75	+0·8705	+0·9794	+1·0738	+1·1517	+1·2122	+1·2565	+1·2867	+1·3058	+1·3288
+1·00	+1·1192	+1·2243	+1·3128	+1·3832	+1·4357	+1·4723	+1·4959	+1·5100	+1·5251
+1·25	+1·3679	+1·4693	+1·5520	+1·6153	+1·6605	+1·6904	+1·7087	+1·7190	+1·7288
+1·50	+1·6166	+1·7143	+1·7914	+1·8481	+1·8867	+1·9110	+1·9249	+1·9323	+1·9387
+1·75	+1·8653	+1·9593	+2·0310	+2·0816	+2·1144	+2·1338	+2·1444	+2·1497	+2·1537
+2·00	+2·1140	+2·2045	+2·2710	+2·3158	+2·3435	+2·3590	+2·3669	+2·3706	+2·3732
+2·25	+2·3627	+2·4496	+2·5112	+2·5508	+2·5740	+2·5863	+2·5922	+2·5948	+2·5964
+2·50	+2·6114	+2·6949	+2·7517	+2·7866	+2·8059	+2·8156	+2·8199	+2·8217	+2·8227
+2·75	+2·8602	+2·9402	+2·9125	+3·0231	+3·0392	+3·0467	+3·0499	+3·0511	+3·0519
+3·00	+3·1088	+3·1854	+3·2336	+3·2605	+3·2737	+3·2795	+3·2818	+3·2827	+3·2831

batch of twins, and that the mean of the batch is only 95 I.Q. points, the standard deviation being 15 I.Q. in both groups (cf. Sandon 1957, 1959). And suppose further that, for the selection at 11+, we pick out the 14 per cent of the total population having the highest I.Q.'s. This implies that the selection is made at about 1 S.D., above the mean, i.e. at an I.Q. of 115. The mean deviation of the children thus selected will be the average of the tail above this borderline. This indicates that this mean will be 1·53 S.D. above the general mean, i.e. the average I.Q. of the selected group will be 123. But for the twins, however, a cut at 115 will be 20 points of I.Q. (i.e., 1·33 S.D.) above their own mean. Interpolation in the table shows that the mean of the selected twins will be at 1·80 S.D. above their own mean, or 1·47 S.D. above the general mean, i.e. at an I.Q. of 122. Hence even the selected group of twins will be, on the average, poorer than the group selected from the population as a whole.

As a further example, let us take an instance from the field of ' subjective probability '. In a recent investigation on the estimation of success in trials depending largely on luck Professor Cohen (1960, p. 140) found that his grammar school pupils estimated their chances of being right in their guesses for certain test problems would be as follows: (relative ' frequencies ' given in fractions): Very Unlucky, 0·07; Unlucky, 0·17; Realistic, 0·52; Lucky, 0·17; Very Lucky, 0·07. If we assume the underlying variable to be normally distributed, these proportions would correspond to the following borderlines between successive categories: $-1{\cdot}47$, $-0{\cdot}71$, $+0{\cdot}71$, and $+1{\cdot}47$ of S.D. The widths of the corresponding categories will thus be ∞, 0·76, 1·42, 0·76 and ∞. And, on interpolating in the table below we find that the means of the five corresponding sections of a normal curve would be $-1{\cdot}91$, $-1{\cdot}04$, 0·00, $+1{\cdot}04$, and $+1{\cdot}91$. We infer therefore that Cohen's subjects on an average, distinguished more clearly between ' Realistic ' and ' Lucky ' (or ' Unlucky ') than between ' Lucky ' and ' Very Lucky ' (or between ' Unlucky ' and ' Very Unlucky '). Moreover, the effect of the adverb ' Very ' on the various estimates was far greater when it was applied to the category of ' luck ' than it appeared to be with Cliff's subjects when the adjectives thus qualified related to personal qualities; with Cohen's subjects the multiplying factors are 1·91, 1·04, and 1·84, as compared with Cliff's three values of 1·317, 1·254, and 1·323 (Cliff, 1959, Tables XI–XIII).

Editorial Note. As the formula used by Mr. Sandon does not appear in the more familiar textbooks, and as the references cited include no proof, the following proof is appended, based on an early lecture of Karl Pearson.

The equation for the normal curve may be written

$$z=\frac{1}{\sigma\sqrt{(2\pi)}}\exp\{-x^2/(2\sigma^2)\},$$

Differentiating we obtain,

$$dy/dx=-\frac{1}{\sigma^3\sqrt{(2\pi)}}x\exp\{-x^2/(2\sigma^2)\}.$$

Now let $\bar{x}$ be the mean (the abscissa of the centroid) of a vertical slice or section bounded by the ordinates, x_1 and x_2. Then by the definition of such a mean,

$$\begin{aligned}(q_1-q_2)\bar{x} &= \frac{1}{\sigma\sqrt{(2\pi)}}\int_{x_1}^{x_2}\exp\{-x^2/(2\sigma^2)\}x\,dx \\ &= \sigma^2\left[\frac{\exp\{-x^2/(2\sigma^2)\}}{\sigma\sqrt{(2\pi)}}\right]_{x_2}^{x_1} \\ &= \sigma^2(z_1-z_2),\end{aligned}$$

where (as in the text) z_1 and z_2 denote the ordinates at x_1 and x_2, and q_1 and q_2 denote the areas of the curve cut off by those ordinates. If we put $\sigma^2=1$, we then obtain

$$\bar{x}=\frac{z_1-z_2}{q_1-q_2}.$$

In dealing with problems like that described on p. 118 (Table I) and in this *Journal* (XIV, p. 11) research students in psychology at University College have found it simpler to use a conversion-table in which the ' lower boundaries ' and the ' width of the sections ' are given in percentages: the standard measures required are given in the body of the table. As in all such tables, interpolation is needed; and the values so obtained are therefore only approximate. The final and more accurate figures must be calculated directly from the equation just given. Roneo'd copies of the table are obtainable from the laboratory. Mr. Sandon hopes to publish a similar table and other examples in a later issue.

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THE GIFTED CHILD

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The following paper examines certain arguments recently advanced to show that the range of individual differences due to genetic selection is too slight to be of practical importance, and in particular that the abilities of gifted individuals are the result of their early environmental opportunities rather than of their genetic constitution. In reply two new pieces of evidence are brought forward: (i) data demonstrating the wide range of variation to be found among children reared under very similar conditions; (ii) after-histories of persons of high ability whose environment offered an impediment rather than aid to their intellectual development. An estimate is made of the relative frequency of gifted individuals in different socio-economic classes, and suggestions are put forward for improving methods of selecting gifted children of high ability.

I. Current Criticisms

The conclusions I have ventured to draw about gifted children, both in earlier publications and in more recent contributions [2, 3, 4, 5, 7], have of late been subjected to considerable criticism, based, it would appear, quite as much on misunderstanding as on genuine disagreement. Several writers[1] have complained (in the words of one of them) that " the highly complicated algebraic proofs offered by Fisher, Burt, Howard, and others are quite incomprehensible to the average teacher and educationist, and probably to most psychologists. Could not ", it is said, " a few straightforward arguments be given without all these mathematical complications? " Most of the critics follow the line taken by Dr. Fleming, Dr. Floud, Dr. Heim, and Mr. Maddox [11, 12, 14, 16]. The commonest contention is " that the role attributed to heredity by members of the Galton school was greatly exaggerated, and that individual differences in ability or aptitude are due far more to social and environmental influences than to genetic conditions ". This certainly is a criticism that can be met without entering deeply into technical details or embarking on elaborate statistical analyses.

But first of all there is one preliminary objection, raised most frequently by teachers, which applies quite as strongly to the arguments of my critics as it does to my own. " Why," asks Mr. Somerville, " do psychologists treat the problems of the supernormal as if they were of the same general type as the problems of the subnormal ? Both issues seem to be regarded as no more than special aspects

[1] See below pp. 171f. The editors regret that it is impossible, for lack of space, to print all the letters received on the subject of the present paper. Of those omitted more than half endorse the main conclusions reached in the articles cited, and are concerned either with minor criticisms or with supplementary inquiries. So far as possible these are also dealt with in the following pages.

of the broader and vaguer problems of 'general intelligence'... We have books on 'the backward child' and books on 'the mentally deficient', but no book on 'the gifted child' as such. It is principally for these reasons that the question has been so widely ignored by the public at large." If Mr. Somerville is thinking primarily of practical problems, I should readily agree[1]. The first task of the psychologist, however, is to deal with the underlying theoretical issues—in particular with the question whether subnormal and supernormal ability are mainly acquired or largely innate; and this demands an inquiry into the assumption of an underlying 'general capacity', commonly termed 'intelligence', and into the degree to which variations in this capacity, whether above the average or below it, appear to be determined by genetic tendencies.

Mr. Somerville's view seems to be that geniuses and defectives form rare and isolated groups, standing quite apart from the ordinary population. This is an ancient notion, still widely held by members of the general public. But it is surprising to learn (as both correspondence and examination scripts reveal) that it is a view which is also shared by many teachers, and even by lecturers at training colleges.

The first to point out the manifest errors which such a conception involves was Sir Francis Galton; and it may be helpful to recapitulate some of his simpler arguments here. To begin with, he adduces plausible reasons for believing that, except for occasional pathological cases, the basic differences between individuals are differences of degree. "From the village idiot to such illustrious geniuses, as Shakespeare or Newton, every grade and shade of variation is to be found. And the distribution is wholly continuous, like that for stature and other bodily measurements. There are no gaps, no gulfs." Secondly, he argues, none would deny the occurrence of cases of innate dullness and deficiency; and, since all we know of inherited or inborn characteristics indicates that their distribution is symmetrical, it is reasonable to infer that the high abilities of the geniuses and near-geniuses are also innate.

As stated by Galton, and reformulated by biometricians like Pearson and his coworkers, these elementary facts and the argument based on them raises a strong presumption in favour of the theory that high abilities are mainly innate. But in the absence of more direct empirical evidence, they of course remain very far from conclusive. Let us therefore look a little more closely at the detailed evidence.

II. The Inheritance of High Ability

Galton's principal data consisted of the pedigrees of about 1000 men of genius, which were elaborately analysed and compared. "The general result," he says, "is that one-half of the illustrious men have one or more eminent

[1] In the reports and memoranda on practical procedures which I prepared as psychological adviser to the London County Council the two sets of problems were in fact treated quite separately. The problems of the 'supernormal' or 'gifted' were dealt with in reports on the scholarship system and on methods of selection for secondary schools.

relatives ". The chance that such a man will have an eminent son proves to be well over 200 times as great as the chance for a parent chosen at random, with similar odds in decreasing proportions for remoter degrees of kinship [13]. To Galton's argument in this general form there is one obvious criticism, repeated by a number of our correspondents, namely, that the opportunities enjoyed by the child of a gifted parent will be far more favourable to the development of high intellectual achievements than those enjoyed by the child of a parent chosen at random. Galton of course anticipated criticism along this line, and attempted to forestall it. However, since his major interest lay in the direct inheritance of ability, he was chiefly occupied with cases in which the children *resemble* their parents; and this naturally invites the retort that environmental influences can produce such resemblances quite as easily as genetic influences.

The Mendelian theory, however, maintains that genetic influences may be responsible not only for resemblances between members of a family, but also for their differences. To the staunch environmentalist one of the most puzzling phenomena is the appearance of extremely able individuals in households where both the intellectual and the material conditions would, one might imagine, condemn every child to hopeless ignorance and inefficiency. Accordingly, instead of looking, as Galton did, for instances of brilliant sons born to brilliant fathers, it will, I suggest, be more instructive to note the numerous instances of brilliant sons born to parents in the humblest circumstances. Carl Gauss, generally acclaimed as " the greatest mathematician of all time ", was the son of a German bricklayer ; Laplace, almost as illustrious, the son of a farm labourer in Normandy; Marlowe, Winckelmann and James Mill were sons of cobblers; James Watt, Whewell, Opie, Lincoln and Carlyle, sons of carpenters; Thomas Cromwell, David Cox, and Faraday, sons of blacksmiths; Defoe and Wolsey, sons of butchers; Luther, Zwingli, and John Knox, sons of peasants; Kant's father was a strap-maker, Tieck's a rope-maker, Franklin's a soap-boiler, Bunyan's a tinker, Kepler's an innkeeper, Cardinal Richelieu's a soldier of the guard; Chatterton's ancestors are described as ' unskilled workers, mostly sextons'. And so one might go on[1].

The detailed accounts of the early development of these and many other budding geniuses fully confirm the inference suggested by this brief list. Unfortunately the illustrative cases commonly described in educational textbooks (cf. [17] and refs.)—Macaulay (I.Q. 180), the son of the governor of a colony who was also a F.R.S., Galton (I.Q. 180), the grandson of a celebrated zoologist and botanist, brought up in a cultured home and taught by an almost equally brilliant sister, John Stuart Mill (I.Q. 190) whose father, leader of the new

[1] The data for British persons I have taken partly from Havelock Ellis's *Study of British Genius* [10], but mainly from the *Dictionary of National Biography*. For one or two cases the information is slightly conflicting: e.g. some accounts say Wolsey's father was a grazier, others a wool merchant—presumably he kept sheep and sold both their flesh and their wool—but all seem agreed that he was just " an honest poor man ". For foreign personages I have relied on similar publications from the countries concerned, and especially on Miss Cox's volume in *Genetic Studies of Genius* [9].

Utilitarian philosophy and author of the standard *History of India*, acted as the boy's personal tutor and gave him a profoundly erudite education—these are all cases in which the child was brought up in well-to-do circumstances among highly cultivated relatives and acquaintances, where every opportunity and motive were provided for precocious scholastic progress. Such examples, chosen no doubt because they were the most dramatic that could be found, nevertheless leave the impression that the child's exceptional intellectual attainment may be largely the result of his exceptionally favourable environment.[1] It would, I contend, be far more instructive to examine biographical records where the young genius was brought up in relative poverty by parents who neither enjoyed nor encouraged intellectual interests.

Of the older scientists known to fame Kepler has left fairly detailed descriptions of his early life, which can be checked and amplified by other contemporary documents. His father, who with his wife kept a tavern in a small town in Swabia, was continually running off to the wars: he was, says his son, " a vicious and quarrelsome vagabond, who treated his family very ill, narrowly escaped hanging, and finally went into exile and disappeared ". Fortunately, the Dukes of Würtemberg, after embracing the Lutheran faith, had provided a scheme of scholarships " for children of the poor and faithful " whose abilities qualified them for a university education; their intention was that the state should produce erudite pastors who could defend the new creed against hostile attacks. In this way young Kepler was enabled to attend advanced courses in theology, classics and mathematics. After graduating at Tübingen at the early age of 20 he was recommended by the senate for a post as teacher of mathematics at Graz, and so was enabled to embark on his investigations and publish the volumes which have secured him his niche in the hall of fame.

Even more instructive is the case of Gauss. He was born in a miserable cottage in Brunswick. His father, a bricklayer, intended his son to follow in his steps and provide for his old age, and consequently " did all in his power to thwart the boy's intellectual interests, and prevent him going off to acquire an education suited to his talents ". Fortunately at the local school there was a

[1] In support of the view that " genius is the product of environment " Mr. Harrison cites the Fellows of the Royal Society—chiefly at its foundation " members of wealthy and noble families ", like Lord Brouncker the first president, the Duke of Buckingham and the other seven peers, Sir Robert Boyle, ' father of chemistry and brother of the Earl of Cork ', or else close friends of the Court like Pepys and Evelyn. In point of fact the most noted of the original Fellows were neither noblemen nor ' close friends of the court '; they were men of a humbler origin like Newton and Wren, Oxford professors of puritan sympathies who held chairs from which the royalists had been evicted, and even people like Wallis, the mathematician, who had applied his science to breaking the ciphers of the royalist army. In the next generation, when two earls held the presidency in succession, hardly any work of note was carried out by the Fellows (now said to be nearly all " gentlemen of noble bloud ") and the *Philosophical Transactions* were not even published. At this period the ' scientific geniuses ' came from an entirely different social level—like Watt, the son of a carpenter and shipwright, Brindley, the mechanic who had never learnt to spell, and the members of the Lunar Society at Birmingham or the Philosophical Society at Manchester—people such as Priestley, Jenner, Herschel, and the like.

young assistant with a passion for mathematics. He and the ten-year-old pupil worked together after school hours on algebra and mathematical analysis, questioning the current proofs of the binomial theorem and several of Euclid's demonstrations. When Gauss was only fourteen his remarkable accomplishments came to the notice of the Duke of Brunswick, who paid for his further education. At sixteen he started to develop a non-Euclidean geometry; and at eighteen, so he tells us, he discovered the method of least squares.

Or, to come nearer home, consider Faraday. His father was a journeyman blacksmith, his mother " industrious but uneducated ". Michael was one of a family of ten. Like them he attended a common day school, but learnt little more than reading, writing, and arithmetic. At thirteen, to augment the family earnings, he obtained a job as errand boy for a bookbinder, who afterwards took him on as apprentice. In this way he had a chance to supplement his meagre education by studying the volumes, particularly those on science, which he found on the shelves of the bookshop. A customer gave him tickets for Sir Humphrey Davey's lectures at the Royal Institution; and here he attracted the attention of Sir Humphrey, who eventually appointed him lecture-assistant.[1]

Even when the future genius is born into a family which can provide both opportunity and encouragement, it not infrequently happens that the path which he follows is quite different from that which might have been expected from his social opportunities or his parents' wishes or plans. James Bernouilli took as his motto *Invito patre sidera verso* (" against my father's will I study the stars "); and the biographies of eminent people are full of similar deviations.

As an example let me choose one who has been described as " the greatest man of science that Ireland ever produced ". William Rowan Hamilton, surprisingly enough, remains unnoticed in Galton's collection, in Havelock Ellis's *Study of British Genius*, and in Miss Cox's *Genetic Studies of Genius*. His early education was supervised by his uncle, a curate at a neighbouring village, who was a pedantic and versatile linguist. The uncle's intention was that William should become a parson and if possible a scholarly divine, and towards this end the first essential was to learn the ancient tongues. At five the boy could translate Latin, Greek, and Hebrew, and recite long passages of Homer in the original, as well as much of *Paradise Lost* and *Regained.* By eight he had added French and Italian, and by ten Sanskrit and Chaldee (i.e. the Aramaic in which much of *Daniel* and parts of *Ezra* are written). At thirteen he claimed that he had mastered one language for every year he had lived; and twelve months later he composed and presented a flowery welcome in Persian to the Persian Ambassador who was visiting Dublin. Just before this, however, he had met the calculating prodigy, Zerah Colburn; and (as he relates) resolved to penetrate the secret of the lad's methods, which Colburn himself did not fully understand.

[1] I refrain from citing similar examples from our own contemporaries in the world of science and letters. The stories of politicians who have risen from the humblest origins to cabinet rank—the Thomases and the Morrisons, the Bevins and the Bevans—are too well known to need further mention.

The result, to the uncle's great dismay, was that William now became passionately absorbed in all types of mathematical problems—at first however " purely by way of recreation ". At Trinity College he carried off almost all the available prizes, and obtained top marks and the highest honours in both classics and mathematics. He already detected errors in some of Laplace's algebraic proofs, and had laid the foundation for his discoveries in the theory of optics. When he presented his first memoir to the Royal Irish Academy one of the professors declared: " This young man—I do not say *will* be, but *is*—the first mathematician of his age ". Shortly afterwards at the early age of 22 he was elected professor of astronomy. Nor were mathematics and the sciences his sole pursuits; he still kept up his interest in linguistic subjects, and remained throughout his life a prolific versifier. One could hardly find a more striking illustration of superiority in *general* ability.

Each of the foregoing examples would of itself suffice to refute the view that " the special achievements of men of genius are predominantly the results of the special facilities and stimulation afforded by their environment ". Our universities can show long lists of students whose circumstances must have afforded far better opportunities and stimulation for studying natural science than Faraday's, and who have nevertheless made no comparable contributions to the sciences they studied; and there must have been innumerable schoolboys who have received far more encouragement in their work on mathematics than Hamilton or Gauss, who nevertheless barely got beyond the calculus. At the same time, however, each of the cases I have described demonstrates that, in addition to the boy's own ability and interest, there must at least be some kind of opportunity for acquiring the necessary educational foundations—an opportunity, it would seem, too often the outcome of some happy chance or fortunate condition peculiar to the individual's place and time. How many potential geniuses have failed to develop for lack of such provision it would be rash to estimate.

III. Data from Mental Tests

These biographical particulars will, I hope, supply something of the " plain and concrete evidence " that Mr. Somerville desires. If fuller and more exact confirmation is needed, we must turn to data obtained from more recent research on gifted children tested and studied while still at school. The most obvious way of demonstrating the inadequacy of a predominantly environmentalist hypothesis is to compare children brought up from birth in practically the same material and social circumstances. In the course of our inquiries in various orphanages and residential institutions under the London County Council we encountered from time to time an appreciable number of able boys and girls among the inmates who had been transferred during the earliest years of infancy; and we had free access to the official files containing confidential information regarding their family histories. In spite of the relatively uniform environment the variability

of the children taken as a whole was actually greater than that of the general population, viz. 17·3 I.Q. points instead of between 15 and 16[1]. The average I.Q. was only 91, and there was a high proportion of dull and defective youngsters. But about one in fifty had sufficient ability to merit a junior county scholarship although owing to their comparatively poor attainments in English and arithmetic, many of those who deserved a scholarship failed, or would have failed, to pass the customary examination. Of eight children with I.Q.'s over 130, five were known to be the illegitimate offspring of fathers of a comparatively high intellectual and social status; two others were children of fathers in the professional classes who had died and left the mother in a state of extreme poverty. Plainly, in cases such as these, it would be out of the question to explain the high intelligence of the child by the special cultural opportunities furnished by his home environment, since the child's only home has been the institution.

Several critics, however, have objected that " cases taken from institutions would seem to be definitely atypical, and can hardly be regarded as representing the ordinary run of school children ". Accordingly let us turn to pupils brought up in their own homes and taught at ordinary elementary schools. To eliminate so far as possible differences in environmental conditions, we may take a representative sample of children whose parents have all much the same social and economic status. From the surveys of the school population of a typical borough I will select children of fathers belonging to the highest level of what in official reports are so-called ' semi-skilled ' workers. Above this level we might occasionally find marked cultural or educative influences emanating from the home; and below it there would be an appreciable number of cases in which poverty, irregular employment, or downright neglect might interfere with the child's early development. The distribution of intelligence among children in this group is shown in Table 1.

Table 1. Distribution of Intelligence among Children of Similar Home Conditions

I.Q.	Number	Percentage
Above 130	1	0·1
120–130	10	1·2
110–120	76	8·9
100–110	279	32·8
90–100	297	34·9
80–90	101	11·8
70–80	73	8·6
60–70	13	1·5
50–60	2	0·2
	852	100·0

[1] Among the children attending ordinary Council schools the standard deviation is (as a result of the scaling employed) almost exactly 15. If, however, an allowance is made for children in the same age groups who are either excluded on grounds of mental deficiency or in attendance at private or independent public schools, then the standard deviation rises to rather over 16.

The mean is 96·0, and the standard deviation is reduced from 15·0 to 11·8. Even so, the range is still a good deal wider than we might have expected. If we take the old borderline for junior county scholarships as a criterion, then I should estimate that only 1 or 2 per 1000 in this group would win scholarships as compared with about 2½ per cent in the ordinary population. Nevertheless the group itself is large; and, over the whole county, and during the years in which these and similar surveys were made, quite a number actually succeeded in the scholarship examination or gained equally high marks in the 11 plus examination.

Indeed even in cases where the home circumstances were yet more unfavourable, I have known several pupils who have managed to achieve a highly successful educational career. I have room to cite only a single instance—the son of an illiterate dock labourer and his alcoholic wife, the eldest in a family of five. During the day time, except when at school, he was kept pretty busy running errands for his mother and looking after the family. However, while laid up with a broken leg at the age of seven and a half, he acquired a passion for reading, and later started borrowing books from the public library to read in secret at night. The librarian drew my attention to the boy; I tested him, and found he had an I.Q. of 142. He was transferred to another school, which included what was known as a ' scholarship class '; won first a junior county scholarship, then a University scholarship; and today holds a professorship in science at a Northern university.

IV. Intelligence and Social Class

There has of late been considerable discussion about the relative number of gifted children in the different ' social classes ' or, as I should prefer to call them, ' occupational groups '. The view which prevails among the majority of teachers is apparently that, " since gifted pupils are commonly the offspring of gifted parents, they are naturally to be expected chiefly in the professional classes or the equivalent ranks in commerce or industry ". Social psychologists maintain that this is merely " the superficial effect of the unfair advantages that accrue to members of the privileged and well-to-do classes ". Let us therefore inquire into the actual figures.

Table II. Numbers of Gifted Children in Different Occupational Classes

Class	*Occupation*	*Number tested*		*I.Q. over* 110			*I.Q. over* 120			*I.Q. over* 130		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
I	Higher Professional	74	3·4	52	70·3	9·6	25	33·8	11·5	15	20·3	27·8
II	Lower Professional	330	15·2	136	41·2	25·1	69	20·9	31·8	23	7·3	42·6
III	Skilled	1127	51·9	260	23·1	47·9	96	8·5	44·2	13	1·1	24·1
IV	Partly skilled	356	16·4	69	19·4	12·6	22	6·2	10·2	2	0·6	3·7
V	Unskilled	285	13·1	26	9·1	4·8	5	1·8	2·3	1	0·4	1·9
	Total	2172	100·0	543		100·0	217		100·0	54		100·0

In the course of school surveys undertaken primarily to locate exceptional types of children—the subnormal at one end of the scale and the supernormal at the other—my colleagues and I have obtained detailed information regarding the occupational status of parents in the case of over two thousand pupils attending London schools—a reasonably random sample. Here it will be convenient to keep to the simple fivefold classification introduced by the Census in 1911. I select it, not because it is necessarily the most satisfactory for purposes of psychological or educational research, but because it is the most familiar to the general reader and the most widely adopted in official or semi-official reports. As the definitions of the several classes are expressed in somewhat general terms, I have drawn the lines of division so that the percentage allocated to each class shall be approximately the same as those reported in the Occupation Tables printed in the last available *Census of England and Wales* (1951).

Class I includes persons in higher professional, administrative, or managerial posts; class II persons in lower professional, administrative, or managerial posts (e.g. teachers in elementary schools and those engaged on commercial or industrial work with much the same earnings); class III includes clerks, typists, shop assistants, and foremen as well as skilled manual workers in various trades; class IV consists of ' partly-skilled ' workers; and class V of the remainder—chiefly various types of unskilled labour. In Table II I have given figures for three grades of supernormal children—viz., those forming the top 25 per cent, the top 10 per cent, and the top $2\frac{1}{2}$ per cent of the school population. These correspond fairly closely with those having I.Q.'s of 110 ,120, and 130, or upwards. I choose these proportions, because $2\frac{1}{2}$ per cent represents roughly the proportion of junior county scholarship winners in the early days of such examinations ; they form in fact the type of 'gifted children' for whom we have the most complete records, including a large number of after-histories reaching almost to the middle of adult life; 10 per cent was approximately the proportion allocated to so-called ' central schools '; and 25 per cent represents the average proportion who nowadays pass the 11 plus examination for entry to a grammar school.

Looking along the first row, it will be seen that, out of the 2 172 children who comprised the entire sample, only 74 (barely $3\frac{1}{2}$ per cent—col. 2) were drawn from the higher professional class. Of these all but half a dozen had I.Q.'s above 100; 52 (over 70 per cent—col. 4) had I.Q.'s above 110, and were therefore of grammar school type. Looking down the columns, we see that out of the 543 children of this grammar school type, only 52 (less than 10 per cent—col. 5) were drawn from the higher professional class. Among these 52 there were 25 with I.Q.'s above 120, and 15 with I.Q.'s above 130: thus one in five (20 per cent) of the children in this small subgroup possessed this exceptionally high level of ability (col. 10). Nevertheless, even at this high level it is not Class I but Class II (col. 11) which furnishes the biggest quota; and owing to their great numerical superiority, the three ' working classes ' (III, IV and V) actually supply a larger proportion of children with I.Q.'s over 130 than the higher professional class.

I fancy much of the controversy arises because the various disputants fail to distinguish between the two types of percentage—namely, those reached by comparing the numbers by rows (i.e. occupational classes) and those reached by comparing them by columns (i.e., I.Q.'s). Defining, for the sake of argument, the term ' gifted ' to mean an I.Q. of 130 or more, one could quite truthfully say that a child from class I has a one-in-five chance of being ' gifted ', whereas a child in class II has only one chance in fourteen, and a child from class IV barely one in two hundred. But it is equally true to say that only about a quarter of the ' gifted ' children come from class I. Unless the two statements are carefully worded and explained, they sound contradictory, and may easily lead their respective spokesmen to imagine that their opponents are misinterpreting the facts.

VI. The Ascertainment of the Gifted

Assuming then that these figures may be accepted as plausible estimates and that the differences which they reveal are due largely to genetic factors, some kind of selection becomes essential if (in the words of the Act) we are to provide each child with " such instruction and training as may be desirable in view of his age, ability, and aptitude ". Up to now, as our various after-histories show, at least a third of the pupils in the higher intellectual categories have failed to obtain the type of education which their ability seemed to deserve. Not all such failures are to be ascribed to faulty methods of selection. Motivation is as important as intelligence; and development after 11 plus as important as development before 11 plus—and far harder to predict.

Naturally those who believe that genetic differences are too slight to produce appreciable differences in actual development, and therefore attribute the individual's educational and social achievements chiefly to the advantages conferred by his early environment, would prefer to abolish all schemes of selection. In this country, however, the majority of teachers and educationists have been forced by their own experience to recognize the wide range of innate differences, and consequently appreciate the need for selection at both ends of the scale—for the gifted as well as the defective, for the supernormal as well as the subnormal. Nevertheless even they have freely criticized what they take to be the grave shortcomings of existing procedures. The commonest target of attack is the ' intelligence test ' and particularly its use at 11 plus.

The most uncompromising criticisms, however, emanate from the social psychologists. Dr. Floud and her collaborators maintain that the grammar school tends to become " an undisputed preserve of the middle class ". As they rightly point out, the chance that the son of a non-manual worker (e.g. a clerk) will enter ' a secondary school of the grammar type ' is three times that of a working-class boy. And such differences they attribute largely to the reliance on the ' intelligence tests ' introduced by psychologists for this purpose and to

the influence of environment on the test-scores [12, p. 143]. Dr. Stott's oft quoted protests [18, p. 27 f.] are stronger still: " educational administrators," he tells us, " clamour for I.Q.'s; . . . the legend that the intelligence test measures innate unalterable intelligence[1] was fathered by an earlier generation of psychologists and mothered by administrators only too glad for someone else to carry the baby "; and, since both the educational administrators who " clamour for I.Q's " and the educational psychologists who construct the tests are themselves commonly drawn from the privileged classes, the results obtained tend to select those who, by virtue of their home background and their individual interests, are likely to turn into typical members of the same social class. " The notion of selecting gifted children by administering written tests of intelligence devised for the 11 plus examination is," we are assured, " hopelessly out of date; not only the statistical evidence, but still more the observational data show that their effect is merely to favour a specially privileged type of child—in short, a child in most cases of the educated middle-class type of parent." And a comprehensive survey of the more popular arguments, leading to the same conclusion, has been set forth by Mr. Flann Campbell [8].

Now in point of fact the introduction of intelligence tests for selecting entrants to grammar schools had a very different history from that which these writers assume. The tests to which they refer were in the first instance devised not for practical purposes but for scientific surveys. To gather data on an extensive scale written tests applied in groups were necessary; and accordingly Mr. R. C. Moore and I [1] developed a number of verbal and non-verbal group tests—opposites, analogies, reasoning, code tests, matrix tests, completion tests —types of test which still form the bulk of the so-called 'intelligence tests' used in the 11 plus examination. The main purpose was to eliminate, so far as possible, the effects of environmental differences, and to provide some kind of comparable scale by which the estimates furnished by teachers from different schools with different subjective standards could be objectively equated. In any case it was always intended that the test marks themselves should be submitted to teachers who knew the individual pupils, so that doubtful or discrepant cases could be re-assessed by individual tests and other means.

In Britain tests of this kind were first employed for educational purposes in a series of experimental studies which I carried out as the Psychologist to the London County Council from 1913 onwards. At that time the Council awarded a very limited number of scholarships annually to pupils of 11 plus who showed

[1] This statement is based on a widespread misunderstanding. The view adopted by the ' early generation of psychologists ' who introduced such tests was that there is an innate general factor influencing mental efficiency in all directions, and the word ' intelligence ' was borrowed as a convenient shorthand label. Tests were constructed to aid in assessing this factor. ' Intelligence ' has now become a term of popular parlance with a far broader meaning, and, of course, in that sense is by no means unalterable. The imperfections of tests were emphasized from the very outset, and the fact that ' intelligence ' *as measured by these fallible methods* alters appreciably was demonstrated again and again (see [2], pp. xv, 151 f, 175).

exceptionally high ability, and in doing so was guided by the pupils' performances in a purely scholastic examination consisting of two papers only—English and arithmetic. An analysis of the results showed that the number of ' gifted children ' thus selected varied enormously from one electoral division to another. In Lewisham, Dulwich, St. Pancras North, and Hackney North, the proportion of scholarships gained in 1913 per 1,000 children in attendance at the elementary schools was well over 7 as compared with an average of 2·9 for the whole county; in Bethnal Green, Haggerston, Lambeth North and Southwark West on the other hand, it was less than 1·0, and in Finsbury only 0·2

In the inquiries that followed, the application of intelligence tests such as I had already compiled for researches carried out at Oxford and Liverpool indicated that able pupils coming from the poverty-stricken and semi-illiterate homes in the areas just mentioned were badly handicapped in the scholastic type of examination, particularly in the English paper. Later I was invited to carry out similar investigations with intelligence tests in Bradford, Birmingham, and Northumberland (in the last case with the welcome collaboration of Godfrey Thomson). In London we concluded that the wisest plan would be, not to make a separate paper consisting solely of intelligence tests, but to incorporate a number of problems of that type in the existing papers. This procedure, we believed, would help to frustrate the inevitable efforts at special cramming; whereas the addition of a separate paper called an ' intelligence test ' would, we felt sure, give rise to many misconceptions—as has indeed proved to be the case with the 11 plus examinations.

So far from the new test-questions favouring the child of the ' educated middle-class type of parent ', as our critics allege, the demonstrable effect was to reduce the disproportionate number of scholarships going to the ' privileged few '. Thus in the sample borough in which our most intensive surveys were carried out the proportion of scholarship winners coming from so-called working class homes (those of skilled, semi-skilled and unskilled manual workers) rose from 1·8 per cent to 3·5 per cent.

But, contrary to the prevailing notion, it is not the proper function of a test applied at 11 plus to pick out children of the highest *innate* ability. If at this age a pupil is to be accepted as a fit candidate for a grammar school, he must also possess a certain modicum of elementary educational attainments. The detection of innate ability is a task that should be undertaken at a much earlier age. With youngsters just up from the infants department an intelligence test, rightly selected and rightly applied, can provide a valuable means of discrimination. What is wanted at 11 plus is no longer a test to discover differences that are inborn, but rather an assessment of the child's present all-round competence. Some writers of course use the word ' intelligence ' in that broader sense. But much misunderstanding would be avoided if this equivocal term were dropped in favour of some non-committal designation, e.g. 'general classification test '. As it is, many of the brightest children, particularly those who live in

poor areas and attend schools where most of the other pupils are dull and backward, are apt to end up at 11 plus with attainments in English and arithmetic well below grammar school requirements.

One of the earliest results of mental testing was in fact to demonstrate how grossly the abilities of the more gifted pupils are underestimated. Of children with I.Q.'s over 130 as many as 54 per cent—well over half—had educational quotients under 120. Of those with I.Q.'s over 120 only 11 per cent had E.Q.'s over 120, and 6 per cent had E.Q.'s below the general mean. On an average " the abler children are in effect deprived of more than half the educational equivalent of their natural advancement—over 10 per cent of their mental age " [2, pp. 177f].

Nevertheless I fully agree that the test-papers in actual use could be greatly improved. Dr. Stott rightly complains that different psychologists and different tests often produce widely varying I.Q.'s for one and the same child. Part of the trouble arises from the fact that all too frequently the tests employed are never adequately validated. It looks so easy to construct an ' intelligence test ' for the 11 plus examination by inventing sets of opposites, analogies, and problems involving completion, codes, or syllogistic reasoning; and the local authority's examiner no more thinks of trying them out beforehand than he would think of trying out his questions in English and arithmetic.[1] And even tests that have been adequately constructed and validated may have very different standard deviations. Thus a child with an I.Q. of 125 on Dr. A's scale may have an I.Q. of 135 on Dr. B's. An education official familiar only with Dr. A's would infer from B's mark that the child was the brightest in 200, while an official familiar only with B's scale would infer from A's mark that the child was merely the brightest out of 10 or 11. Those who discuss I.Q.'s should always state what particular test-scale they have in mind, and not refer, as they so commonly do, to ' *the* I.Q.' as though all acceptable tests would give the same figure.

But there is a further defect. The tests which are easiest to construct and standardize are tests which turn on logical analysis, aided by a retentive memory —particularly those which can be formulated in verbal terms. Now a large proportion of highly gifted persons carry out their thinking by non-verbal means, and belong to what I have termed a synthetic, intuitive, or productive type rather than to the analytic, logical, or reproductive. They are quick at seeing problems in the concrete, and they reach their conclusions by implicit thinking rather than explicit [1]. Among adults this is true, not only of artists, musicians, inventors, and technologists, but also of nearly all who are marked by unusual originality and creative capacity. As I have pointed out elsewhere [6, pp. 19f.] an excessive

[1] In compiling a group test for the examination in one educational area the examiner decided to use an ' opposites test ' of his own: to avoid previous familiarity he excluded all the items that appeared in his model (test 21 in *Mental and Scholastic Tests*). The result was that most of the items he included consisted of those which we had thrown out in the course of our preliminary item-analysis because of their low validity.

analytic bias was a serious fault in Terman's method of selection. The gifted individuals picked out by his tests were in later life conspicuous chiefly for their achievements in the world of academic science: there was not a single painter, sculptor, or musical composer with a reputation comparable with those of the dozen most eminent scientists. Even in their literary productions their success lay rather in academic textbooks and scientific or technical articles than in poetry, *belles lettres*, or other branches of imaginative writing.

I believe then that if improved methods of testing and checking tests were introduced, if special abilities and disabilities could be taken into account, and if character and motivation could also be assessed (on the basis for example, of teachers' reports), the accuracy of the selection—the ' predictive value of the examination ' as Mr. Harrison terms it—would undoubtedly be increased. However, the shortcomings of the present methods of selection have, I fancy, been much exaggerated. In the inquiries carried out by the National Foundation it was estimated that every year about 78 000 are ' wrongly ' allocated; and the figure has been pounced on by the critics. But, as the investigators point out, comparatively few of the ' wrong ' allocations are *seriously* wrong. In any case the inevitable fluctuations in growth and interest and the unforeseen accidents of the future put *perfect* accuracy quite out of the question. The real object of the system of selection is, or should be, to make certain of catching in the net the ablest pupils of all—say the top 3 per cent. If at eleven plus you select only 3 per cent, you will miss more than half of them. To be reasonably sure of including all of them when your validity coefficient is only 0·80 or thereabouts, you must select the brightest 25 per cent according to your tests. You will then probably miss only 1 in 400 or 500 out of the brightest 3 per cent.

So far, the types of gifted children I have been discussing, namely, pupils with I.Q.'s of 110, 120, or 130 and over—are by no means rare, though somewhat infrequent; they comprise roughly speaking, those who are the ablest out of 4, out of 10, or out of 50 respectively. Galton's definition of a genius, however, was the ablest person out of 4000, which means one with an I.Q. of 160 or more. Such an individual would differ as much from a first class honours candidate as the latter does from the average skilled workman. And here, I think, the point to underline is the increased spacing in the differences between individuals as the individuals depart more and more from the mean. Suppose we mark a random sample of a hundred children on a uniform scale from 0 to 100. The difference between the 50th and the 51st would be almost imperceptible (50·0 marks and 50·5); but the difference between the brightest of all and the next brightest would be 10 times as great, namely, 96 and 91 marks. It is this increasing rarity towards the top which makes the greatest geniuses—the Shakespeares, the Raphaels, the Newtons, and their like—stand out as almost inexplicable miracles of nature.

Now the interest of the educationists has been concentrated for the most part on selecting and educating pupils with relatively moderate degrees of giftedness. But our richest source of intellectual power lies in those who are

the most exceptionally gifted; and these really need an education quite distinct from that provided by the ordinary grammar school or college course. In the past such an education has, in rare instances, been provided by a parent (as in the case of John Stuart Mill); more frequently it has been worked out by the gifted individual himself (as in the case of Galton), aided in certain cases by one or two early strokes of luck (as in the case of Faraday). In a country like the United Kingdom, with a population of about 50 million, there must be at least 12 000 geniuses, in the sense defined by Galton. I estimate that the talents of more than half of them are virtually wasted—largely owing to the inadequacy of our methods of selection and training.

Mr. Harrison complains that " the educational system Professor Burt would have us establish implies a society in which class-distinctions, instead of being reduced or superseded, are sharply accentuated: according to his view it would mean a community arranged in a hierarchy of *intellectual* grades, presumably with a glorified public school for the intellectual élite at the top; according to the view accepted by most contemporary sociologists it would create or perpetuate a community arranged in *social* grades based on income, privilege, or sheer intellectual snobbery ".

There is confusion as well as misconception here. In suggesting " something like a new type of public school " for those endowed with an I.Q. of over 160 my aim was to avoid the constant sense of frustration and maladjustment that so many of them experience while living at home or attending a school of the ordinary type whether primary, secondary, grammar, or public.[1] They need the companionship of those who are their intellectual equals among children and the discipline of those who are their intellectual equals among adults. But it was certainly not my wish to transform public schools into institutions for an " intellectual élite ". Naturally, in reporting results of researches on tests of intellectual ability, I have made little or no reference to the non-intellectual aspects of the problem; and among educationists generally I fancy a preoccupation with selection by examination has led to a similar narrowing of their scholastic outlook. Critics of the public schools and of " class distinctions in the educational system " seem to believe that " professional parents who prefer to send their children to public schools attended by boys from the same exclusive social class as themselves " do so in order to " purchase a superior type of instruction given by a superior type of schoolmaster with only a dozen boys in a form instead of thirty or forty". This is a common, but to my mind a very inaccurate, picture. The reason why the parent from the professional classes sends his son to a public school, often at the cost of drastic financial sacrifice, is not so much a desire to secure a better type of intellectual education (in many respects the intellectual education is often inferior to that given in the nearby secondary schools established and equipped by the local authority) it is rather that he wants the training in character, manners, speech,

[1] As is evidenced by the autobiographies of so many of them: cf. [19], for example.

and social ideals which, with few exceptions, only the better type of public school at present seems able to impart.[1]

To my mind the disparity in traditional ideals is by far the most important difference associated with class distinctions. As those we questioned continually pointed out, the professions have always prided themselves on their high ethical and social ideals handed down from the past. The medical profession has its code of unselfish service which allegedly goes back to Hippocrates and is enforced by its Council; the legal profession claims to stand for strict and impartial justice; the clergy naturally aim at upholding moral standards; the teaching profession seeks to include among its aims—and has done since the days of Plato—both character training and intellectual integrity, inculcated by example as well as by precept: and finally in this country both politicians and civil servants inherit a tradition of loyalty and public spirit. The code of the average professional family claims to incorporate these various ideals and naturally such parents want their sons and daughters to associate with children from families who cherish the same high principles. In my view these parental aspirations should also be taken into account by our methods of selection.

Be that as it may, we still know too little about the needs and potentialities of gifted individuals at successive stages of their mental growth and of the various ways in which their abilities, interests, and temperamental qualities develop or mature. What is urgently needed is a fresh series of longitudinal case-studies, starting with an intensive investigation of a broad sample of gifted children while still at school, and resumed at intervals until the children have attained the middle of adult life. The inquiries carried out in the London County Council's schools may serve to indicate what is practicable.[2] But both the numbers studied and

[1] This type of motive emerged very clearly in an investigation carried out by Miss J. Whittaker and me into the reasons given by parents in seeking particular types of school for their children: it appeared most conspicuous among parents of the professional class.

Mr. Harrison also objects that I " appear to claim for the gifted group a complete freedom from every type of blemish, physical as well as mental ". This presumably is meant as a rhetorical exaggeration. It should, however, be emphasized that I was making no ' claim ', but merely reporting ascertained facts—viz. that the percentages showing physical defects are smaller among the gifted group than among the general population. But I mentioned one striking case in which the percentage was actually higher—namely, visual defect (chiefly, at the age in question, moderate myopia). Terman's figures show the same difference (20 per cent more or less defective among the gifted, only 16 per cent among the control group; 11 per cent of the gifted wearing glasses, only 5 per cent among the control). Terman does not, however, realize the problem presented by this odd exception to the general rule. How is it to be explained? Has short sight acted as a reason for preferring books and school work to games, or is the difference the result of natural and sexual selection?

[2] Many local education authorities have now been using standardized ' intelligence tests ' in the 11 plus examinations for fully ten of fifteen years. It should therefore be possible to make comparative studies (similar to those carried out on a more limited scale in London) of pupils who have passed or failed. The after-careers of large numbers in various parts of the country, both urban and rural, could be followed up at least to the beginning of the University stage, i.e., to the age of 18 or thereabouts. May we hope that psychologists or research-students will be encouraged to collate and analyse the vast mass of data thus available, and, where possible, obtain the comments of the teachers who still remember the pupils in question?

the information sought were far too limited, and referred to conditions very different from those obtaining today. If the investigation is to last for some thirty or forty years, it should be undertaken not by some single individual, however enthusiastic, but by some scientific or official body. At least one of the investigators should be a member of the local authority's staff: only in this way will they be able to secure the requisite cooperation from teachers, school doctors, social workers, and parents, and to obtain the necessary facilities for carrying out physical, psychological, and educational tests and for studying the confidential data already obtained. The knowledge achieved by such an inquiry would amply repay its cost.

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NOTES AND CORRESPONDENCE

THE STRUCTURE OF THE MIND. I

A Note on Physical and Psychological Space

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Professor Burt has brought a number of objections against my formulations of the representative theory of perception and of the nature of the mind-brain relationship. He says first of all that, in common with Dr. Levy, I have " taken the highly ambiguous concept of ' space ', and have applied it to very disparate sets of phenomena." The chief ground for this criticism is apparently that Dr. Levy has suggested that his own ' all-embracing space-time ' may be regarded as a kind of ' factorial space '. This leaves him open to the objections that Professor Burt raises. However, I have never myself suggested that either perceptual space or the space that includes both physical space and perceptual space, are factorial spaces. In my book[1] I based the concept of space on the relations between after-images and other sense-data, that is, on something that is given in experience and depends in no part on mathematics. Indeed Nicod[2] bases geometry itself on his observation of such relations as these.

Professor Burt then goes on to question my use of the concepts of ' sensible ' space and ' physical ' or ' scientific ' space. Each individual, he says, has not just one ' sensible ' space, but several—e.g., a visual, an auditory and a tactual space, each separate; and moreover these are not so much ' spaces ' as ' fields ', i.e., limited in extent. Secondly, he states that my concept of ' scientific ' space is really a ' psychological construction ' rather than equivalent to the ' sophisticated concept of modern physics '.

These difficulties may best be avoided if we pay more attention to the entities concerned instead of basing our discussion on the difficult concept of ' space '. If we do this, we can make the following analysis of the underlying situation. Each individual consciousness manifestly consists, as Professor Burt notes, of a collection of events ; and some of these events are organized into fields—e.g., the visual field of visual sense-data and the somatic field of somatic sense-data. These are *spatial* fields : the individual units which can be isolated from the total field concerned manifestly bear spatial relations to other units in the same field. However, this is not the whole story, as Professor Burt observes here and elsewhere[3]; for there are other contents of consciousness, such as olfactory and gustatory sense-data, as well as thoughts and feelings which do not have spatial properties. Nevertheless it is still true to say that *some* mental entities have spatial properties (or that some mental events have spatio-temporal properties). And it is this key fact about the nature of the mind that the Cartesian theory fails to recognize.

The next step in my defence is to make it clear that my theory does not really deal with the space of physics. It deals rather with the physical objects of common-sense—chairs, houses, trees, etc., and with their mutual spatial relations. The theory can be put simply as follows:

[1] J. R. Smythies, *Analysis of Perception*, 1956.

[2] Jean Nicod, *The Foundations of Geometry and Induction*, 1930.

[3] Cyril Burt, *J. Soc. psych. Research*, XLI, 1961, pp. 55–60.

1. Physical common-sense objects bear spatial relations to each other, and they are located in one common spatial continuum; that is, any one object could, in principle, *touch* any other physical object.

2. One man's visual sense-data bear spatial relations to each other and are located in another continuum—his visual field in his consciousness ; that is, each of his visual sense-data is contiguous with other sense-data or with other visual sense-data which in turn are contiguous with other visual sense-data, and so on; and this process exhaustively accounts for all the sense-data in his visual field. It will be noted that we here depend on basically simple topological relations, for it is these that the contents of consciousness manifest most clearly.

3. The essential step now is to ask what are the relations between sense-data and physical objects. Clearly they are not spatially contiguous with each other in the way that physical objects touch other physical objects, and that sense-data are contiguous with other sense-data. Professor Price's theory answers this question by saying that there are no spatial relations between sense-data and physical objects, but only causal relations ; the total universe of events consists of a physical world of common-sense objects, and, in addition, a number of mental worlds—one for each individual. It is in our description and explanation of the *behaviour* of common-sense objects that the concept of the space of physics arises. Each mental world consists of that individual's sense-data (both the spatial and the non-spatial kind), images, thoughts, etc. Thus consciousness is not *merely* a ' special type of relation or interaction '[1]. It does not consist *solely* of my awareness of sense-data, although, of course, this awareness is an essential feature of consciousness. Included in the class of events that comprise each individual consciousness are the sense-data themselves and their behaviour. Professor Broad's theory differs from Professor Price's theory only in postulating that the physical world and the various mental worlds form different cross-sections of a common n-dimensional manifold.

In my own analysis the problem of the relation between the visual field and the other components of consciousness becomes no longer a matter of mind–brain relations, nor one for physiologists to worry about ; it becomes a problem for pure psychology with introspection as our only source of information.

I will conclude by dealing with two minor points. (i) Physicists have only assumed that events " which act as causes and the results which ensue as their effects are and must be events in the same space ", because they have never supposed that there might be other space-time systems *outside* the confines of the world of physical objects. But, as Ducasse has shown,[2] there is nothing in the logic of causality that permits us to state on *a priori* grounds that events in one space cannot be causally linked with events in another space or even with non-spatial events. Causality in this context means no more that whenever events a, a^1, a^2, . . . a^n occur in the brain, then events b, b^1, b^2, . . . b^n, occur in the owner of that brain's consciousness. (ii) My use of television as a model for the representative theory of perception specifically excludes any need for the detail of the mechanical arrangements (such as scanning) to be the same in both cases. The model is a logical one, and illustrates the essential features of a non-symbolical representative mechanism such as I suppose the human visual mechanisms to be.

[1] Cyril Burt, *loc. cit.*

[2] C. J. Ducasse, *Nature, Mind and Death*, 1951.

THE STRUCTURE OF THE MIND. II

To the Editor, British Journal of Statistical Psychology

Sir,—I should like, if I may, to add my support to the case made out by Dr. Taylor, Mr. Hansel, and other contributors to your pages for a purely objective psychology. Such a psychology, as they rightly argue, should be based on the same essential concepts and adopt the same empirical methods as every other branch of natural science. It would, as Dr. Taylor more especially has maintained, keep well within the bounds of what he calls ' Newtonian science ', it should retain a rigorously mechanistic or deterministic attitude, since that is the only way to avoid the illegitimate assumption of subjective qualities and occult entities, such as ' ego', ' psyche ', ' consciousness ', ' mind ', and the other elusive concepts so dear to the philosophical psychologist.

Professor Burt, if one may judge from his recent papers, would have us resurrect the traditional Cartesian notion of a simple unextended substance, called a mind or a soul. He seems to take it for granted that the concept of mind is an empirically established theory. In that case one is bound to ask what exactly is the observational evidence on which the proof of the theory is presumed to rest. In the view of most psychologists, however, as Watson and his followers have made us realize, such a conception explains nothing and is entirely redundant. Professor Burt apparently believes that he is able to identify some such entity by ' introspection ', i.e. by direct intuitive self-observation. That merely illustrates how unreliable introspection can be as a scientific method. In science observation means observing things which any other investigator can also observe and check. Few psychologists claim to have observed any such thing as a mind.

Now, unlike many of the critics I have mentioned, I myself would not go so far as Watson in his famous manifesto and deny the very existence of consciousness. But I should certainly agree that the recognition of something strictly private and personal, going under that name, could not possibly justify the postulation of an exclusive entity called ' mind ' to account for it. The reintroduction of such a term into current psychology (as its champions frankly admit) implies the acceptance of the metaphysical concept of substance—a concept which irrefutable criticism has long since exploded. In the place of substances could we not substitute events ? A flash of lightning is a brief event; a human body just a longer event; and a mountain a very prolonged event. Thus what McDougall regards as a mind or a mental structure I should regard merely as a mental process. This, if I understand them aright, is the view taken by Russell and Whitehead; and, as they point out, it effects a manifest economy in the number of basic concepts we are led to postulate. Consciousness in any case is surely not a thing or a stuff but an event or process. As such it is incessantly changing. The causes of the changes are cerebral. Certain physical events in the brain are the necessary and sufficient causes of all conscious events. This is after all the only interpretation that is consistent with the general theory of the universe adopted in modern science.

Let us recall the other mystical principle that for so long confused the lowlier biological sciences—that of ' life ', to which Aristotle gave the same name, ' psyche '. Today every biologist would unhesitatingly reject the concept of a unique ' principle of vitality ' or ' life ', and trace the characteristics of living matter to the peculiarities of certain complex chemical molecules evolved by natural selection. In the same way, I maintain, we should, in company with most psychologists inspired by a modern biological outlook, reject the parallel concept of mind as a ' principle of consciousness ', convinced that, like life, it too will turn out to be merely an emergent property of the still more complex patterns in which other chemical molecules are organized—the outcome once again of the blind processes of evolution. If a label be required for this modified type of behaviourism, I suggest the term ' epiphenomenalism ' as defined (except for a slight verbal modification of my own)

by Professor Drever: " the theory that the chain of causality is complete on the bodily side, so that conscious phenomena are merely the accompanying effects, and make no real difference to the overt behaviour ". K. S. Hudson

Sir,—The recent discussions[1] between Dr. Smythies, Dr. Levy, and Professor Burt have brought once again to the fore the problem of the philosophical dualism which psychology by its very title seems to imply. For the statistical psychologist the issue is of fundamental importance: are the factors and other variables with which he is concerned to be described in terms of their alleged conscious content (according to the practice of introspectionists like Spearman) or in terms of behaviour (as most contemporary American factorists prefer to do)? Burt evidently regards his factors as ' factors of the mind '; and in a series of articles he has reviewed the results obtained by such methods with the object of showing that what they reveal is essentially ' the structure of the mind '. The phrase has been echoed by several later statistical writers. Vernon shortly afterwards published a volume on ' The Structure of Human Abilities ' and Eysenck one on ' The Structure of Human Personality '. The altered phraseology is perhaps meant to evade any dispute over the use of the word ' mind '; nevertheless the words ' ability ' (which goes back to Galton) and ' personality ' (a favourite term with Ward) still indicate leanings towards the older introspectionist nomenclature rather than any change to a strictly behaviouristic viewpoint.

Burt and most of his students (Moursy, Banks, Howard, Gregory—to take only contributors to this journal)—in common with Thurstone in his earlier work—still cling to the term ' mind '. And yet, so far as I am aware, neither Burt nor any of the others have explained what precisely they understand by the word. In his interesting paper on ' Hebrew Psychology ' (this *Journal*, XIII, pp. 195f.) he discusses the *original* connotations of terms like ' spirit ', ' soul ' and ' mind ', which have played so prominent a part in the historical development of psychology; he himself seems still to think of mind in the old scholastic fashion as a specific kind of stuff—to borrow a favourite phrase of William James; and the description tends rather to encourage students to conceive or picture each individual as carrying round inside him a kind of supersubtle body with a structure of its own—a ' pneumatic soma ', in the Greek phrase he quotes, in short, a veritable ' ghost in the machine '. According to him, the ' mind ' or ' self ', is not, as it is with Descartes, merely a simple unextended substance; it is something of which in introspection I am directly aware.

If it is argued that ' mind ' is a hypothesis, then it is, I submit, a hypothesis for which (in Laplace's phrase) there is no necessity. Thanks to advances of the basic sciences the scientific world-view has become increasingly homogeneous. Occam's razor—" entia non sunt multiplicanda "—should by now have assured us that for the scientist the universe contains physical entities and processes, and nothing but physical entities and processes. It follows that, since mind by definition is not physical, minds do not form part of the furniture of the universe. " All sentences of psychology," says Carnap, " describe physical occurrences, to wit—the bodily behaviour of human beings or other animals." Since psychologists are now generally agreed that psychology is simply ' the study of behaviour ', and since all that we observe is essentially behaviour, why cannot Professor Burt rest content to conform to the practice of his contemporaries, and describe the conclusions of factor analysis and statistical psychology *in terms of behaviour*, without dragging in a hypothetical entity like the mind, which after all is from its very nature unobservable? Can he give us any specific instances of individual differences that cannot be completely described in behaviouristic terms? T. H. Bolton

[1] J. R. Smythies, ' Three Classical theories of Mind ', *J. Soc. psych. Res.*, *XL*, 1960, pp. 385f.; cf. Burt, *ibid.*, *XLI*, 1961, pp. 55f.; and Levy and Burt, this *Journal*, *XIV*, 1961, pp. 51f.

THE STRUCTURE OF THE MIND

A Reply

By Cyril Burt

I. Differences in Methodology

The Definition of Mind. The primary object of this paper is to plead for a reintroduction into psychology of the concept of mind. In support of that plea, I shall endeavour to explain in fuller detail what I take to be the nature of mind, and at the same time try to answer my various critics. Hitherto, when summarizing the conclusions reached by researches on statistical psychology—for instance, in *The Factors of Mind* and articles on 'The Structure of the Mind'—I have used the word without stopping to define what precisely I mean by it. In an earlier work[1] I briefly mentioned that I "used the term as a convenient label to designate the hypothetical basis of consciousness, whatever it may be"; and added that we ought rather to refrain from deciding in advance whether it was or was not material, or spatially extended, or even identifiable with certain parts or tissues of the brain. It seemed to me that any detailed description of the mind was a matter to be settled not by definition at the outset, but by deductions step by step from the conclusions reached as our investigations advance.

The Principle of Parsimony. The question with which most of my critics begin is whether the assumption of an immaterial mind is 'really necessary'. Dr. Bolton, for example, inquires "why, since psychologists are generally agreed that psychology is the study of behaviour, and since all that we can observe is essentially behaviour, Professor Burt cannot rest content to adopt the practice of his contemporaries, and express his conclusions solely in terms of behaviour, without dragging in a hypothetical entity like the 'mind', which from its very nature is unobservable?" My answer is that nowadays few scientists outside the ranks of psychology accept the methodology by which Dr. Bolton supports his arguments—that of the operationist, the logical positivist, and the devotees of Occam's razor. Certainly none would dream of banning a useful theoretical concept just because it is unobservable. After all, even in the material sciences, what the physicist and the chemist actually observe is behaviour. But they do not (as Dr. Bolton demands) "rest content with expressing their conclusions solely in terms of behaviour". No one could 'observe' the nucleus of an atom; yet that did not stop Rutherford from postulating such an entity to account for the behaviour of alpha-particles when fired at a thin film of gold or silver foil; nor did it prevent later physicists from depicting the unobservable nucleus as an aggregate of just so many particles (or rather quasi-particles) and endowing it with an entirely new kind of 'binding energy'. I claim a similar freedom for the psychologist.

Dr. Bolton, however, cites Carnap, and contends with him that "Occam's postulate should have convinced us that, for science at any rate, the universe contains physical and nothing but physical entities; it follows" (he continues) "that, since mind is not a physical entity, minds are not part of the furniture of the universe". It is precisely at this point that our views diverge. If he and other behaviourists and physicalists were content to apply Occam's razor simply as a working principle, I should readily agree; but, when they tell us to accept what it dictates, not as a working principle, but as an empirical premiss, I demur. In any case, so far from becoming "increasingly simple and increasingly

[1] *How the Mind Works* (1933), pp. 35f., cf. pp. 331f.

homogeneous ", as nineteenth century physicists and Dr. Bolton have maintained, the " scientific world-view " has, during the last sixty years, become increasingly complicated.

But what I chiefly wished to protest against in the articles which my various critics cite was the behaviourist's regular practice of regarding certain types of elusive or anomalous problems as already solved by direct deduction from their ' homogeneous world-view ', and dogmatically denying the need for any *ad hoc* verification of the facts they allege. Mr. Hudson rejects all problems requiring introspective study. Mr. Wilson rejects any theory or reported observation implying the efficacy of will. Mr. Hansel assures us that it is a waste of time to seek or sift evidence for telepathy because the laws of mechanics preclude any such thing. Dr. Moseley, and, if I understand him rightly, Mr. Gregson, " reject the notion that in the course of conscious experience we may directly apprehend certain objective qualities called values ", because value can not possibly form an objective element in a physical universe governed solely by physical laws. And in much the same spirit Dr. Taylor and Dr. Levy recommend the psychologist to " keep well within the bounds of Newtonian science, since that is the surest way to avoid the invalid postulation of subjective qualities or occult entities, like consciousness and mind ".

But science never attains finality. As its history shows, whenever men have cited the current scientific picture of the universe as sufficient in itself to dispose of any novel theory or unexpected observations, they have nearly always been wrong. Consider how, right down to the eighteenth century, the occasional cranks who believed that the singular properties of magnets and of amber might be worthy of study were persistently ridiculed as " credulous time-wasters ". Consider how even Newton's theories were challenged because they " revived the occult doctrine of action at a distance, which is no better than an outworn superstition ".[1]

The Need for a Concept of Mind. In citing Occam's razor—' Entia non sunt multiplicanda '—Dr. Bolton omits the final phrase ' praeter necessitatem '; and it is my contention that there is an inescapable necessity for the concept of mind. One undeniable fact has to be faced by every psychologist, though behaviourists seem almost wilfully to close their eyes to it. In our everyday conscious life each of us is indisputably aware of certain phenomena—our experiences of colour, sound, warmth, pain, pleasure, and its opposite—which cannot possibly be reduced to terms of purely physical or mechanical processes; hence physicalism or mechanism cannot possibly be the *whole* truth about the universe. And, even if we grant that such experiences are mere ' epiphenomena '—transient appearances produced by certain specifiable physical processes, nevertheless the laws or principles relating the physical processes to the psychical experiences cannot themselves be physical laws.

My chief concern therefore has been to affirm the need for some hypothetical framework which, unlike that of the prevailing behaviouristic schools, should not rule out all reference to the contents or the processes of *consciousness* from the very start. For example, in the articles on factor analysis which my critics cite,[2] the test results there summarized seemed to indicate that individuals differ significantly in a wide variety of capacities which can only be described in terms of conscious experience, e.g. capacities for understanding

[1] Leibniz, for example, wrote: " That one body should attract another with no intermediary means is not so much a miracle as a sheer contradiction; for 'tis to suppose the body can act in a place where it is not " (*The Leibniz-Clarke Correspondence*, ed. H. G. Alexander, 1956, p. 72). Locke similarly contended that " it is impossible to conceive that a body should operate on what it does not touch " (*Essay concerning Human Understanding*, II, viii, 11: cf. Aristotle, *Physics*, VII, 242 *b*). In a later edition of his *Optics* Newton endeavoured to circumvent such criticisms by declaring: " These Principles " (gravity, etc.) " I consider not as Occult Qualities . . . but as general Laws of Nature " (*Query* 31).

[2] ' The Structure of the Mind: A Review of the Results of Factor Analysis ' I & II, *Brit. J. Educ. Psychol.*, XIX, pp. 100–111 and 176–199.

meaning, relations, values, for generalizing, for reasoning, etc., etc. Not one of these can adequately be described in purely behaviouristic language, or in the anatomical, physiological, and biochemical terminology employed by the neurologist;[1] and attempts to treat colour-blindness, eidetic images and the like, as no more than modes of behaviour have resulted in a very misleading presentation of the facts.[2]

The Mind as a Model. Mr. Hudson, however, seems quite to misunderstand my position when he suggests that I have put forward my concept of mind as " an empirically established theory ", and asks " what exactly is the observational evidence on which [my] proof is presumed to rest ". All I sought to outline was a very tentative and flexible concept, put forward as a provisional working model rather than a descriptive ' theory '. As to the precise nature of the model proposed there seems to be a considerable degree of misunderstanding. The views that Dr. Bolton and Mr. Hudson attribute to me are views that I should repudiate as vigorously as they do. Mr. Hudson regrets that I should be " resurrecting the traditional Cartesian notion of a simple unextended substance, called a ' mind ' or ' soul ' "; and Dr. Bolton, recognizing that in an earlier paper I had expressly said that I did not regard the mind as " necessarily unextended ", complains that I am " encouraging people to conceive or picture each man as carrying around with him a kind of super-subtle body with a structure of its own—literally a ghost in the machine ". I realize that a word like ' mind ' is apt to call up irrelevant associations of this kind. But in point of fact the type of entity I had in view was neither a ' super-subtle body ' nor a ' simple unextended substance ', but something very different—namely, a ' field '.

And if I am asked—" Why have a model at all? Why not be content with a purely formal statement of your hypothesis?"—then I answer that the use of interpretative models has become part of the practice—and a very fruitful practice—of every modern scientist. The Rutherford-Bohr picture of the atom, with its heavy central nucleus and lighter electrons revolving round it like planets round a sun, was a typical scientific model; like all such models it was avowedly put forward as an abstraction capable of a variety of modifications, and in the end was modified almost out of existence. According to Lotze, " every successful hypothesis is based on an analogy ". The analogue or model supplies the puzzled investigator with a familiar system whose structure and working are already well understood. And in using analogues to construct our tentative models we rely on what Keynes[3] calls the ' known positive analogy ', and admit the existence of a ' known ' (and in all probability an ' unknown ') ' negative analogy ', i.e. features in the analogue which are inapplicable and which we therefore treat as inessential. The quantum physicists' ' particles ', for example, are not intended to be taken literally, since they are quite unlike any particles we encounter in our everyday experience. The physicist is even prepared to adopt several different models for the same situation according to the particular aspect or question that interests him at the moment. The behaviour of any atomic nucleus which contains more than two neutrons presents problems too complex to be solved by direct attack; so the physicist

[1] Dr. Bolton asks for " specific instances of individual peculiarities that cannot be completely described in behaviouristic terms ". I have space only for one or two from my own personal experience. It so happens that my right eye perceives colours rather differently from my left (they have a somewhat stronger tinge of yellow or brown); Dr. Bolton could not possibly infer this peculiarity from my behaviour. Or again, two of my weakest capacities are spatial comprehension and mechanical retentiveness: I find no one ever seems to guess this from my actual behaviour. In answering tests for such capacities I quickly translate the problems into forms more suited to my personal habits and experience, and thus obtain marks which are deceptively high. And the same is true of certain types of deductive reasoning, where I often get more easily confused than most people unless I mentally change the logical scheme.

[2] E.g., J. L. Fuller and W. R. Thompson (*Behavior Genetics*, 1960) have endeavoured to treat all problems of mental inheritance as problems in the inheritance of behaviour.

[3] J. M. Keynes, *A Treatise on Probability*, ch. xix, ' The Nature of Argument by Analogy '.

falls back on various models none of which forms a perfect analogue—the liquid drop model, the Fermi gas model, the nuclear shell model, and so forth. And, since present day psychology seems in much the same confused condition, I see no reason why psychologists likewise should not exploit a number of different models simultaneously instead of quarrelling about the merits of different schools, or propounding yet another school " to end all schools ". For certain purposes I should be quite satisfied to adopt the behaviourist's model; for others I should feel obliged to construct a model of my own—yet even that model, I suspect, would prove to be something of a composite.

II. Current Views

Mind as Substance. Dr. Hudson criticizes " the re-introduction of ' mind ' into psychology " because, he says, " the notion of a ' mind ' implies the acceptance of the metaphysical concept of substance—a concept which irrefutable criticism has long since exploded ". The word ' substance ', however, has been used in a variety of ' metaphysical ' senses; and Dr. Hudson fails to explain which particular meaning he himself attaches to the term. From his other remarks I suspect that what he is thinking of is, not so much the more technical usages to be found in the writings of the philosophers or scientists, but rather the narrower sense which the word so often bears in popular speech—" the tangible material out of which a commodity is made ", to quote the dictionary definition.[1]

Explicitly or implicitly all the natural sciences use the traditional concept of substance, even if they do not employ the word; and they use it chiefly as a matter of convenience, without committing themselves to any specific metaphysical doctrine. My own usage was much the same. However, since Dr. Hudson has now thrown down a challenge, a fuller explanation seems needed.

To avoid the misunderstandings which inevitably arise when one introduces popular terms—with their trail of popular associations—into scientific expositions, it will be necessary to fall back on a somewhat esoteric terminology, so that words employed can be unambiguously defined. Let us begin by distinguishing, within the sum total of reality, two broad categories: (i) " particulars, which enter into complexes only as the subjects of predicates or as the terms of relations, and which, if they belong to the world of which we have experience, also exist in time "; (ii) " universals, which occur either as predicates "

[1] In its more precise sense the word ' substance ' (a Latin word coined by analogy with the Greek) is a term of logical exposition rather than of metaphysics. It goes back to Aristotle, who interprets the concepts of ' substance ' and ' attribute ' by reference to the logical notions of ' subject ' and ' predicate ': " a primary" (i.e. concrete or individual) "substance", he says, " is that which is neither asserted of a subject nor present in a subject" (*Categ.*, 5, 2a, 11). The later ' metaphysical concept ' is traditionally defined by saying that a substance is *res per se subsistens*, i.e. " an individual self-existent entity characterized by a certain degree of unity and of permanence in time, and capable of standing in causal relations ". McTaggart, who criticizes the notions of both cause and time, suggests an alternative formula: " a substance is that which has qualities and is related, without being itself either a quality or a relation or having qualities or relations among its parts " (the last clause is added to exclude ' facts '). His wider phraseology is intended to take in certain relatively transient ' particulars ', which many philosophers would not regard as substances, e.g., a sense datum (J. E. M. McTaggart, *The Nature of Existence*, I, p. 68; cf. *id.*, *Contemporary British Idealism*, I, p. 253). The usual objection (which presumably Dr. Hudson has in mind) is that, as traditionally interpreted, substance turns out to be a mere peg on which to hang qualities or attributes, just a " something I know not what ". Many writers treat the word ' substance ' as synonymous with ' thing '. But a ' thing ' is usually considered to be not only persistent in time, but also bounded in space—a condition which can only be fulfilled at temperatures similar to those ordinarily obtaining on the earth: in the centre of the sun ' things ' (as thus defined) would be impossible. Moreover this restriction would forbid our conceiving either energy or Cartesian mind as ' substance '.

(i.e., as " attributes ") " or as relations, and do not exist in time ".[1] The terminology and the definitions are Russell's. However, as Johnson has pointed out,[2] the word ' particular ' may give rise to awkward side-issues: for, when a quality or relation is predicated of a ' particular ' subject, it is tempting to argue that the quality or relation must also be in some sense a ' particular '. I shall therefore adopt the alternative name ' existent '; and I shall follow Johnson in assuming that " the category of the ' existent ' may be divided into two subcategories: (i) what continues to exist or the ' continuant ', and (ii) what ceases to exist or the ' occurrent ', every occurrent being referable to a continuant ". " A ' continuant ', " as he says elsewhere, " continues to exist and remains identical with itself, although its states or relations may be changing." His instances include the psychological as well as the physical. " The body which illuminates the earth is identical with the body that warms it," and " the person who was experiencing the toothache is identical with the person who now intends to go to the dentist."

The statement, therefore, that mind is a ' substance ' need mean no more than this: first, that mind is not a ' universal ' (i.e. not a quality or a relation, as consciousness is often held to be), but rather a ' particular ', or, as I should prefer to say, an ' existent '; secondly, that it is not an ' occurrent ' or a mere ' event ', but a ' continuant ', i.e., an existent that evinces some degree of persistence in time: by this I mean that, although its states and relations may be constantly changing, the modes of change consistently exhibit some kind of regularity and law, such as is usually described by assigning so-called ' dispositional properties ' to an underlying ' existent '.

Mind as Process. Several writers have attempted to dispense altogether with the traditional concept of ' substance ', and have substituted certain types of ' occurrents ', which they term ' events ' or ' processes '. As Dr. Hudson implies, this effects a manifest economy in the number of basic concepts that we postulate to account for the universe as we know it; and, largely for this reason, Russell in his later writings " treats as fundamental the notion of ' events '." By an ' event ' he means " something occupying a finite amount of space-time ", e.g., " seeing a flash of lightning ". Events may have parts; but minimal events have no parts.[3] Things (i.e. pieces of matter), and minds, and indeed " everything in the world ", are ' constructed out of events '. Broad prefers to talk of ' processes '. His favourite example is a noise, which he holds may be adequately described without reference to anything causing the noise or anyone hearing the noise. And he argues at some length that processes may be ' absolute ', i.e., may exist without relation to any substance which may cause them or in which they may occur.

In that case, if we use the word ' mind ' at all, it would designate, not a persistent underlying substance or structure, but the process or processes making up an individual's conscious life-history. On this basis Broad has tentatively developed a form of epipheno-

[1] B. Russell, ' On the Relations of Universals and Particulars ', *Proc. Arist. Soc.*, XII, 1912, pp. 23-24; cf. also *id.*, *Problems of Philosophy*, chaps. ix and x: here we are told that " Particulars *exist* and Universals *subsist* " (a phraseology borrowed from Meinong). In his later writings, however, Russell argues that " there is no need to assume particulars (usually regarded as located in time and space) as subjects in which qualities inhere " and that in their place we need only postulate " bundles of qualities ". But to my mind he has not succeeded in overcoming the difficulties which this alternative formulation involves—difficulties which he himself had pointed out in the article just cited.

[2] W. E. Johnson, *Logic*, Pt. II, p. xi; cf. Pt. I, p. 199. The word ' existent ' may itself be a little misleading. Although it has the form of an adjective, it must not be taken as designating an attribute. I myself should hold that ' existence ' cannot be defined, though it may be indicated.

[3] *Outline of Philosophy* (1927), pp. 287f. This view, as he explains, was originally due to a discussion with Whitehead. In his later writings, however, he tells us that "there is a possibility of a further stage of analysis in which events are no longer the *ultimate* raw material ". With the theory as thus qualified I myself should largely agree, though my " further analysis " would probably be different from Russell's.

menalism. He holds that " we can take the notion of ' mental event ' as fundamental ", and replace the traditional notion of a ' mental entity ' by something consisting solely of mental events and the relations between them.[1] This doctrine goes back to Hume, who in a well known passage declared that " they are the successive perceptions only that constitute the mind. There is no simplicity in it at one time nor identity in different. I venture to affirm that it is nothing but a bundle or collection of different perceptions. I never catch *myself* at any time without a perception, and never can observe anything but the perception ".[2] Much the same doctrine seems still to be regarded as the orthodox psychological theory of the day by many physiologists, by most medical men, and by those psychologists who still use such terms as ' mental ' and ' conscious '. And in the same spirit Dr. Hudson argues that " what McDougall regards as a mental structure [he, Hudson] would regard merely as a mental process ".

The Inadequacy of the Process Theory. It was this view that I sought to criticize in the discussions to which Mr. Hudson refers. For certain limited purposes—particularly those of the medical psychologists and the physiological research-worker—it may no doubt furnish a rough and ready working model. But for the student of general psychology, concerned specifically with the fundamental features of our conscious life and its underlying laws, it is in my view not only grossly inadequate but seriously misleading.

To begin with, it is difficult to think of any convincing instance of a process existing in its own right. If noise means the sound I hear, it ceases to exist when I stop my ears; if it means the physical stimulus, that exists only as a vibration of the atmosphere. Thus noise, and, so far as I can discover, all other processes appear merely to be constituents in a composite situation which contains one or more substances as their basis; and, if our theorists reply that these substances can in turn be reduced to bigger processes, then, simply from a blind belief in parsimony, we seem to be abandoning a most useful distinction and removing ourselves further away from the facts as we actually observe them. Secondly, a substance, as we have seen, has a certain degree of unity and permanence. The silver medal which I saw in my drawer this morning is the same as that which I received as a schoolboy sixty years ago: it has changed a little; it is somewhat tarnished and slightly worn. But if the medal I received was just a ' process ' then, even supposing the ' process ' that occurred sixty years ago appeared almost identical with the ' process ' that occurred this morning, I still could not call it the *same* process. The slow *changes* that have taken place in the meantime no doubt form a ' process ', and the process may claim some degree of continuity. But the word change is meaningless unless there is a relative permanent something which changes. Underlying the manifest changes, science seeks—or at any rate postulates—some kind of latent *invariant* in terms of which to formulate the regularities or ' laws ' that govern the changes. Thirdly, most philosophers, I fancy, and certainly Broad, hold that processes themselves possess no dispositional properties.[3] In a textbook on chemistry, for example, we find enumerated under the heading of each element its physical and chemical ' properties ': we are told that copper will melt at 1084° and boil at 2310°; that, like other metals, it will conduct heat and electricity; and so on. These properties are regarded as in some sense inherent in substances; and it would seem very far-fetched to treat all such statements as really referring simply to ' processes ' that take place in the void.

[1] Cf. *The Mind and Its Place in Nature* (1947), pp. 587f.

[2] *A Treatise of Human Nature*, Bk. 1, pt. iv, ' Of Personal Identity '.

[3] It might in certain cases be possible to modify the ordinary conception of ' events ' or ' processes ' so that they did possess dispositional properties. Mr. Hudson's illustrations—" an explosion is a brief event rather than a substance, a human body is just a longer event, and a mountain a *very* prolonged event "—suggests that he has something of this kind in mind. But the final result would be, not to eliminate the concept of substance, as I have defined it, but simply to give it a new name: instead of a ' continuant ' it becomes a continuous ' event ' (a phrase which is almost a contradiction in terms). And the whole contention turns into a purely verbal issue.

But in point of fact hardly any scientists—not even the physiologists and psychologists who want to reduce *mental* substances to ' processes '—seek to apply the same kind of reduction to *material* substances. The neural processes which condition mental phenomena are almost invariably regarded as processes taking place within a material structure or substance, namely, the brain.

Epiphenomenalism. If, with the physiological materialists, we treat the brain and its various properties as fundamental and the processes of consciousness as a mere stream of transient events, resulting from ' changes in the brain-substance ', then we are naturally led to some type of ' epiphenomenalism '. In its commonest form the doctrine of epiphenomenalism may be summed up in terms of two simple propositions. (1) Physical events in the brain produce conscious events in accordance with certain definite causal laws, some of which have already been discovered and well established; but (2) conscious events play no part in the production either of (*a*) physical events or (*b*) other conscious events. The causative relations between brain and consciousness are thus strictly unilateral.

Now it is not difficult to show that the adoption of a one-sided theory of this kind entails several consequences which are unquestionably paradoxical if not mutually incompatible.

(1) Let us begin with the first of the two propositions that make up the theory. It implies that certain physical processes, taking place in a piece of matter called the brain, constitute the necessary and sufficient causes of all conscious processes. This view is put forward as " the only explanation which is consistent with the general theory of the universe adopted by modern science ". But almost at the outset it issues in a flagrant inconsistency. One of the basic principles of modern science is the so-called law of the conservation of energy. Now in producing changes of consciousness the physical changes in the brain must either expend energy or not expend it. If they expend it in this particular way, then that amount of energy must be completely lost, since conscious changes can never restore the energy, for by hypothesis they have no influence on physical processes. If, on the other hand, no energy is expended, we are faced with the paradoxical phenomenon of observable changes being produced without energy being needed for their production. Such a phenomenon is found in no other field of science.

But let us waive this familiar objection as a mere *argumentum ad hominem*, and consider the proposition on its merits. In every other branch of science, when a causal process is postulated, there is always some degree of generic likeness (though not perhaps of specific likeness) between the cause and its effect. The most perfect instances of causal explanation are those cases in which mechanical causes produce mechanical effects, as when one billiard ball imparts its motion to another. Even those who question whether causation demands *complete* resemblance between effect and cause nevertheless accept the principle of continuity in some weaker form. They would argue that, if the *whole* cause is not of the same generic type as the effect, at least part of it must be: from which it would follow that at least part of the cause of a mental effect must be mental, or (to put it in another way which some would prefer) the material change provides the occasion for the mental change rather than the complete cause. If material conditions together with certain material changes formed the sole cause of a conscious experience, then this would be ' creation ' not ' production '. It would be, as Stout puts it, " the miracle of making something out of nothing ".[1]

To overcome this paradox the materialist commonly tries to suggest that consciousness is in some way unreal, or even to deny it any sort of existence. Hobbes long ago referred to conscious occurrences as mere ' apparitions ' or ' phantasms '—a metaphor echoed by Professor Ryle when he jeers at the ' ghost in the machine '. Huxley and the reflexologists brush them aside with a string of mixed metaphors: they are " the mere collateral by-products of the proper functioning of the brain . . . a foam, a froth, a phosphorescent

[1] For a fuller exposition of the argument see G. F. Stout, *Mind and Matter*, pp. 116f. and J. Wisdom, *Problems of Mind and Matter*, pp. 83f.

emanation, the noise ground out by the neural machinery in the course of its working, the shadow thrown by the train as it moves on its journey "—in short " epi-phenomena " in the literal sense—" mere superfluous appearances ". And yet if the pronouncements of our consciousness—our first-hand observations and the rational inferences based upon them—are all unreal, what grounds have we for asserting that anything is real?

(2) The second proposition involved in the epiphenomenalist hypothesis is that conscious events, though caused by physical events, do not themselves act as causes: they produce neither physical changes nor changes in other conscious processes. This again is a denial of what seems so obvious to common sense—a denial made solely to preserve the oversimplified picture of the universe that is supposed to be part of ' modern science '. And once again in belittling consciousness the critic is belittling the source of his own concepts. For where after all do we get our notion of causation, if not from the direct experience we seem to have of causation in conscious volition? When we ' will ' make to a movement of our own, we seem to be immediately aware of a specific tendency issuing in a corresponding effect. By analogy we infer a similar process of causation when one material object appears to produce movement in another. The stimulation of the sense-organs of the body is invariably followed by corresponding sensations; therefore, says the epiphenomenalist, the sensations must be caused by the stimulation. But in precisely the same way the conscious decision to make this or that movement is invariably followed by stimulation of the corresponding bodily muscles. Had we no preconceived theory to support we should unhesitatingly infer that the muscular contractions must be produced by our voluntary efforts. The natural inference is rejected only because it is assumed that a genuinely scientific account of the universe must depict it as subject to a rigorous causal determination which leaves no room for anything like freedom of the will. But now that the majority of physicists have discarded the nineteenth century notion of rigorous causal determination the main ground for denying the natural inference has disappeared.

The materialist, however, not only contends that conscious processes have no effect on bodily processes; he also insists that they have no influence on subsequent conscious processes. This again flies in the face of what seems to be common sense. If we can trust the prima facie evidence of our everyday experience, we constantly encounter cases in which one conscious event produces or affects another. And indeed we often seem directly to see not merely *that* the conscious consequents follow from the conscious antecedents, but even *how* they follow; and we see it far more clearly than we see how one mechanical change follows from another. My consciousness of the premisses of a deductive argument, for instance, appears definitely to cause me to draw, consciously and explicitly, the logical conclusion; and social intercourse would become quite impossible unless we assumed that the perceptions, beliefs, and interests of experiencing individuals are directly operative as effective conditions determining the subsequent course of their thoughts, emotions, and mental efforts.

Moreover, the mode of determination is itself peculiarly mental. The whole organization of the successive states and phases in my individual consciousness seems to imply a specifically mental type of relation quite different from the types of relation that obtain between mere[1] physical events. Both might be regarded as species of causal determination in a broader sense; but if so, we must recognize an inter-mental type of causation as well as an inter-physical. The fact that this assumption, or rather the hypothetical working models of the mind based on this assumption, serve a useful pragmatic purpose in guiding

[1] I insert the word ' mere ' to forestall objections based on superficial analogies with logical processes carried out by a ' logic machine ' or an electronic computer. In such cases of course the construction and programming of the machines themselves are the result of the designer's conscious deliberations. We are surrounded with machines and manufactured articles, which would never have come into existence unless some ingenious inventor, guided by conscious and rational steps, and prompted by non-physical qualities such as value and meaning, had desired and planned them.

both theoretical research and practical treatment is shown by the successes of psychoanalytic theory. In the past most psychologists took it for granted that one conscious process determined another. But, as Professor Mace has pointed out, few have turned this ostensible fact into an explanatory principle; " Freud seems to have been almost the first to take *mental* determinism seriously as a basic explanation in psychology ".

It is perhaps here more than anywhere else that we come up against the most conspicuous inconsistency in the epiphenomenalist's position. He relies on conscious reasoning to prove his doctrine; and yet the very doctrine that he seeks to prove denies that conscious reasoning has any intrinsic validity. By his own showing the sole grounds for his utterances must be certain purely mechanical processes that have occurred within his brain: all else is illusion. And thus, as Stout observes, " materialism, in undermining common sense, undermines itself ".

So far the arguments I have used have been mainly negative. I wanted first to rebut the behaviourist's assumption that consciousness is at most merely a negligible by-product of the changes in some material existent—the ' organism ' or the ' brain '.[1] My next task must be to offer more positive evidence for assuming a distinct kind of existent, with characteristics of its own, entirely different from those ordinarily ascribed to material substances.

Mental Unity. To begin with, we may note that consciousness displays a peculiar kind of unity which would be quite inexplicable were it merely the effect of a miscellaneous aggregate of simultaneous and successive processes going on in a number of separate cells or synapses within the central nervous system. It presents both a simultaneous and a successive continuity.

Although at any given moment I can see a variety of distinguishable objects around me, nevertheless my visual expanse remains a single and unbroken whole: it displays nothing like the mosaic structure characteristic of the retinal rods and cones whose stimulation is supposed to generate it. And although my other sensory experiences—hearing, touch, and so forth—are disparate in quality both from each other and from my visual experiences, nevertheless they too are all fused together as constituents in a single conscious continuum. For this and similar types of continua the simplest and most appropriate name is ' field '.[2] These various sensory fields fuse into one total conscious field. This, too, is incessantly changing; and yet there is no breach, no gap, no interruption: it flows on smoothly like a never-ending stream. Even the deepest sleep or a period of complete anaesthesia causes no manifest hiatus. Often it is only after we have looked at the clock that we realize we have dropped off to sleep.

In both respects the unity of consciousness is quite unique. There is nothing precisely analogous to it in the material world. The seamless garment can be rent; the flawless globe of glass can be shattered into fragments. Consciousness can be neither cut nor

[1] Moseley cites Dr. Smythies as holding some such view. In his present note, however, (p. 141) Dr. Smythies gives up this position (if indeed he ever held it). With nearly all that he now says I find myself in almost complete agreement.

[2] What I understand by the term ' field ' has, I hope, been sufficiently explained in a previous note (' Field Theories and Statistical Psychology', *Brit. J. Statist. Psychol.*, XII, 1959, pp. 153–164). However, as Mr. Foster rightly observes, I there confined myself to explaining the concept "without giving reasons for making it an essential part of psychological theory ". Mr. Foster is wrong in supposing that " in physics a field always presupposes a material object that produces it—a magnet, a rod carrying an electric charge, a wire carrying an electric current, a body like the earth attracting other bodies in virtue of its mass ". In the theory of relativity it is, so to speak, the structure of the field that is assumed to produce the phenomenon of a body with the power of attraction rather than the material body that is assumed to produce the gravitational or magnetic field. Whether " a psychic field could exist without a brain " I am not prepared to say: there is little evidence to support such an assumption, but I know of no evidence which positively excludes it.

split. And, what is still more curious, the consciousness of one man, though it constitutes a uniform whole within itself, is completely separate and insulated from the consciousness of every other. In this twofold fashion consciousness seems to be essentially *personal*.

No doubt we sometimes talk of ' mental events ' or ' conscious events ', as if they were in some way similar or parallel to distinct ' physical events '. But the plural is misleading. A man's conscious life forms just one continuous event, beginning at birth or a little before, and lasting at least until his body dies. This unity and continuity strongly suggests (as indeed common language almost universally assumes) that the constituent events are related to some permanent and central entity, an entity of a special non-material kind, in short, to a personal self, who, so to speak, owns these events, and refers to them as " *my* conscious experiences or states ", and describes himself by the proper name of ' I '. The only plausible alternative would be to suppose that they cohere into an integral system by virtue of their own intrinsic relations to each other—an alternative which seems rather to restate the situation than to explain it. Now I fancy most introspectionists would agree with Hume that we never *directly* apprehend this central entity or ' personal self ', though many, if not most (as we shall see in a moment) get the impression that there is such an entity somehow implicitly involved in the total situation. Yet, even if we dismiss such vague impressions—which admittedly carry little weight of themselves—it is difficult, as Broad remarks, " to imagine any reason why what is a pure result of system should appear to have a unity of centre. That the mind *does* appear to be of this latter kind ", he continues, " seems pretty certain; and this must be regarded *pro tanto* as favouring Central Theories ".[1]

The epiphenomenalist is forced to account for the unity and continuity of consciousness in terms of the unity and continuity of the material structure of the brain. That is rather like explaining the unity of a symphony concert by the unity of the concert hall in which you hear it. In any case on this anatomical hypothesis it would follow that, if in any individual the living brain could be completely divided into two or more parts, his consciousness would similarly be divided into the same number of parts. McDougall suggested a crucial experiment. " Should I ever be seized by an incurable illness," he announced, " I have asked Sir Charles Sherrington to cut through my corpus callosum with his scalpel, and so separate my two cerebral hemispheres: I hope I shall then be able to say whether I am still a single personality or have become split into two conscious streams."[2]

Surprisingly enough, such experiments have since been carried out, though not on McDougall. " Perhaps the most astonishing of all observations in the sphere of cerebral surgery," writes Dr. Wilkie, " are those made on human beings in whom the whole corpus callosum and anterior commissure have been severed, thus cutting all known direct communications between the left and right hemispheres." One or other of two conceivable consequences were anticipated as a result: " either the patient would report distinct but concurrent activities due to the distinct but concurrent processes occurring in the two hemispheres, or one hemisphere would dominate, while the activities of the other would be suppressed ". Contrary to expectation, neither of these after-effects is reported. Types of activity requiring the cooperation of both hemispheres are still observed. " Binocular fusion and the binocular perception of depth," we are told, " are not materially altered . . . The patient suffers almost no inconvenience (*sic!*) . . . Changes in his behaviour

[1] *The Mind and its Place in Nature*, p. 585. Broad distinguishes two types of central theory: the ' existent ' thus postulated might be " either a pure ego or a central event "—e.g., " a mass of bodily feeling ". He himself, as we have seen, is " inclined slightly to prefer " the latter view.

[2] Cf. this *Journal*, IX, 1939, p. 2.

can be detected only by refined techniques; and these turn out to be rather trivial, consisting chiefly in a lengthening of reaction time."[1]

The Property of Consciousness. Just as we assume that a material substance possesses certain characteristic properties, so, I maintain, we may assume that what we provisionally call a mental substance or ' mind ' also possesses its own characteristic properties. They are essentially ' dispositional properties ', i.e., they can be formulated as general ' laws ' stating that ' *if or when* so-and-so occurs, *then* such-and-such a phenomenon may be observed '. Of these (as I have argued elsewhere) the most distinctive must clearly be the property of becoming conscious in certain particular ways under certain particular conditions.

At this point, however, my critics interpose with an alternative suggestion. " The assumption, " they say, " of a special ' psychic principle ', having the ' dispositional property of consciousness ', is quite superfluous: it is comparable to the obsolete assumption of a special ' vital principle ', having the dispositional property of life." Mr. Hudson goes on to assure us that " to-day every biologist would reject the concept of a unique ' principle of vitality ', and trace the characteristics of living matter to the peculiarities of certain complex chemical molecules evolved by natural selection: in the same way " (he maintains) " we should reject the parallel concept of mind, convinced that, like life, it will turn out to be merely an emergent property of the still more complex patterns in which other chemical molecules are organized—the outcome once again of the blind processes of evolution ".

My answer is that the two assumptions are by no means ' comparable ' or ' parallel '. I find no difficulty in imagining that, at a particular stage of the earth's history, in an atmosphere containing water vapour, methane, ammonia, and the like, and as an effect of strong ultra-violet radiation or of radiation arriving in the form of cosmic rays, certain amino acids might first be formed, including perhaps simple catalysts and autocatalytic products, and that these in turn might by gradual stages combine to produce polypeptide chains of increasing complexity, and eventually throw up macromolecules of deoxyribonucleic acid, possessing the property of self-reduplication and the power to control enzyme synthesis. But Mr. Hudson's version overlooks one important fact: somehow or other the molecules have to get localized and organized into minute and inconceivably intricate structures known as ' cells '. That *this* should be the outcome of the ' blind process of evolution ' (and all within the short space of about one or two thousand million years) I find it far more difficult to imagine. Nor is there anything like the unanimity of view that Mr. Hudson supposes. Walter Elsasser, for example, has argued (in *The Physical Foundations of Biology*) that the facts compel us to postulate certain ' biotonic laws ' (special laws of ' emergence ' I suppose Mr. Hudson would term them) which are incapable of being reduced to the laws of physics and chemistry. And Professor Eugene Wigner has endeavoured to calculate the probability that self-productive systems could come into existence as an automatic result of purely blind or random processes: he claims to show, not merely that the probability is exceedingly small, but that it is exactly zero.

Such arguments, however, can hardly be regarded as conclusive. So let us provisionally admit that what Mr. Hudson says about ' life ' may be true. His conclusion could still have little bearing on the problem of ' mind '. His inference really turns on the ambiguity of the term ' emergence '. The biological materialists to whom he refers see no need for such a word. Given certain conditions at an early stage of the planet's history, then (they maintain) complex substances having the properties of growth and reproduction would in the end automatically be formed, and what we take to be the distinctive characteristics of living matter would in principle (despite Elsasser's contentions) be deducible from

[1] For a more detailed summary of the results, with references to the neurological literature, see J. S. Wilkie, *The Science of Mind and Brain*, 1953, pp. 133f. For reasons that would be here out of place, I should not myself regard such observations as decisive in themselves. In psychology no single type of experiment, however impressive, can be absolutely conclusive, since alternative interpretations are almost always conceivable.

the laws of physics and chemistry.[1] Nevertheless, it remains utterly inconceivable that *consciousness* (in the sense of direct awareness) could possibly be deducible from those laws. Thus in the one case ' emergence ' means ' is in the end automatically formed '; in the other it means ' arises unexpectedly as an entirely new property '. But to stop with the blissful word ' emergence ' is not of itself an explanation. Mr. Hudson's simple answer to the problem of origins resembles Topsy's theory of her own appearance in the world: " 'Specs I just growed ". In all such cases (as the history of electromagnetism and quantum theory sufficiently shows) some ardent scientist would at once start out to discover what fresh concepts or principles were needed to describe the ' emergence ' of these novel phenomena.

Every conscious process or state involves as its essential feature the relation of awareness.[2] But a relation can only hold between two or more terms. Thus in perception there must be a *perceiver* who is capable of perceiving as well as a *percept* which is actually perceived. And I maintain that this is a description of fact, not a mere grammatical convention. The conclusion to be drawn, however, is not that the perceiving is done by a ' Pure Ego '. The ' Ego ' that I myself should infer would be highly impure. Indeed, so far as the present argument takes us, I see no reason why the perceiver should not be the brain or some particular type of tissue within the brain. But in that case the brain or the tissue would be very much more than a mere material substance or a mere succession of physical events: it would itself possess the characteristic properties of what most people would call a ' mind'.

Finally, I hold (as I have argued elsewhere) that in introspection I may become directly aware of this awareness. I perceive that I am perceiving. And here, as we have already seen, the impression that most impartial observers get is that in their own case the perceiving seems to be done by something which has the qualities, not of a palpable substance like the brain, but rather of what the psychologist commonly refers to as a ' self '. Now, when I make a statement such as: " I notice (or ' I judge ') that I am undergoing this or that experience ", the subject—' I '—is mentioned twice; and the form of statement implies that the subject who makes the judgement is the same as the subject who has the experience. Or, again I may say: " I remember that last week I had much the same experience ". And here also the identity of the two subjects is tacitly affirmed. The subject therefore appears to be a more or less permanent entity, namely, what I ordinarily call ' myself ' or ' my mind '.

There are many other ways, more specific than these, in which mental tendencies differ from physical. I will mention only one—the manner in which conscious processes

[1] It is true that Broad, writing in 1925, cited the example of chemical compounds, like water and sodium chloride, as instances of ' emergence '. Nothing, he argued, that we know of either element by itself would give us the least reason to expect that it would combine with the other. Today, however, our increasing knowledge of the inner structure of the atom (notably the theory that valency results from either the transference or the sharing of electrons) goes far to explain not only the phenomena of chemical composition, but also many of the resultant properties.

[2] Many psychologists have denied the suggestion that there is any such thing as a unique and irreducible ' relation of awareness '. Tacitly or explicitly they seem to hold that the relation alleged can be reduced to some other more familiar type of relation—e.g., resemblance, causation, ownership, subject-and-predicate, part-and-whole, a quasi-temporal relation (' compresence '), or (if I understand Dr. Smythies aright) a quasi-spatial relation. I have not discussed these alternative interpretations in detail in the text because I have already examined them at length elsewhere. All seem open to the following objection (as well as several others). Before we can assume that a relation, A, is reducible to some other relation R, we must first show not only (i) that such statements as $s\ A\ o$ entail $x\ R\ y$, but also (ii) that $x\ R\ y$ entails $s\ A\ o$. Those who attempt some kind of proof (or more frequently some plausible type of illustration) commonly stop short with the first of these two propositions; and further reflection almost invariably demonstrates that the second proposition does *not* hold good.

seem directly to modify one another. Our two eyes supply us with two distinct visual fields: in looking through a stereoscope we may see, first, two slightly different pictures of the same scene; then, as the eyes converge, the pictures overlap and finally coincide with a click; the moment they do so, a new sensory quality emerges—what we describe as ' depth '. This experience—the experience of directly perceiving a two-dimensional field as three-dimensional—is so anomalous and unique that it could not possibly have been predicted in advance. It is an effect that has no counterpart among effects of purely physical processes. Moreover, if we make another experiment with one field plain green and another plain blue, the resultant field has an intermediate colour resembling both, namely, greenish-blue. But if one field is green and the other red, the resultant is not greenish-red, but yellow—a colour apparently so pure that the uninstructed viewer takes yellow, and not green, to be the third of the ' pure ' or ' primary ' colours. Introduce yet another overlapping colour—add blue to the green and red (as in the familiar experiment with the triple projector)—and the result is now, not a bluish-yellow, much less a reddish-greenish-blue, but pure white. These and other properties of our mental processes differ so radically from those we discern in material substances, that it seems reasonable to postulate an entirely different type of entity underlying them. To determine in detail what are the various dispositional properties distinctive of such an entity is one of the essential tasks of psychology.

To each of the foregoing arguments, considered by itself, more or less plausible replies could no doubt be given. Taken jointly, their cumulative effect is pretty formidable. Even so, they are still by no means conclusive. Yet that could be urged against the arguments for almost every scientific generalization, and is certainly to be expected in a field where the phenomena are highly complex, and lie away from the well trodden paths followed by the older sciences. Let us therefore now inquire whether it is possible to offer some more positive or constructive theory to meet the various problems we have encountered.

III. Instrumental Theories

Earlier Versions. Among those who reject the traditional notion that the study of consciousness is a " valid and important task for the scientific psychologist ", the majority, as we have seen, have assumed that consciousness is just a collateral by-product " generated by the chemistry of the nervous tissue "—" a mere function of the brain and its workings ". The strongest evidence for such a view is to be found in the close correlation between conscious processes and brain processes. Yet the tacit assumption that the correlation between two such disparate variables is to be explained by a one-sided causal relation between them involves a fallacy with which the statistical psychologist is all too familiar. There are many alternative interpretations which will not only account for the observable correlation between cerebral and conscious processes, but also surmount many of the objections to which a purely materialist hypothesis is open. Of these perhaps the simplest is the so-called ' instrumental theory '. This assumes that the brain is not the generator of consciousness but merely the instrument by which mind or consciousness is enabled to interact with the material world.[1]

As an explicit hypothesis the idea was first mooted by F. C. S. Schiller—one of the livelier critics of the Anglo-Hegelian idealism that dominated academic philosophy in Britain towards the close of the last century. " Matter, " Schiller contends, " is not that which *produces* consciousness, but merely that which *confines its working within certain*

[1] It is curious that C. W. Morris in his *Six Theories of Mind* (1932) fails to include what is ordinarily known as the ' instrumental theory ' among his selection. Bergson is mentioned only as the champion of a novel brand of idealism, which borrows many of the tenets of the ' new realism '. A brief footnote refers to a phrase of his which suggests that the mind uses the body as its instrument, and argues that it would be " more consistent with his thought to say that the body uses mind as its instrument "; and it is in this sense that Morris later discusses what he calls an ' instrumental theory '.

limits . . . Material organization does not manufacture consciousness out of arrangements of atoms, but contracts its manifestations within the sphere which the particular organization of matter permits." Later the theory was taken up and amplified by William James in America and by Henri Bergson in France; and it is in these more elaborate forms that it has usually been discussed and criticized.

The Brain as the Instrument of Consciousness. James assumes that consciousness is a kind of stuff or substance, capable of being divided and reunited like lumps of putty or drops of oil. It exists independently of any material organism, possibly (he says) in the form of " disseminated particles ", which may be temporarily combined into personal form by the brain, but more probably in a " vaster unity " as an " ocean " or " reservoir of consciousness " which can be temporarily subdivided into personal streams. Bergson uses much the same metaphors. Consciousness he describes as " an immense impersonal sea ". Matter in general is " an impeder or obstructor ". But the brain permits a " current " to pass through and organizes it temporarily into a personal form.[1]

Are we then to deny the familiar statement that consciousness is a " function of the brain "? That, as James observes, depends on what is meant by function. The functions of material structures may be of more than one type: they may be (i) *productive* (as when the boiler of an engine produces steam from water); (ii) *permissive* or releasing (as when the trigger of the cross-bow releases the string, and permits the energy stored up in the bow to discharge the bolt); or (iii) *transmissive* (as when the keys and stops of an organ transmit the air to the pipes). When physiologists describe ' thought ' (i.e. consciousness) as " a function of the brain ", they tacitly assume, says James, that it is a function in the first of these senses: the material structures of the brain are supposed to ' create or engender ' certain effects, and consciousness is one of them. " My own thesis," he writes, " is this: when we talk about ' Thought ' as a function of the brain, we are entitled to consider *permissive or transmissive functions.* " According to his view therefore the function of the material brain is to transmit limited amounts of consciousness from some non-material reservoir, thus permitting it to interact with the physical world. The more developed the brain, the richer will be the flow.

James, it will be seen, is here apparently adopting a modified version of transpsychism. The doctrine of *Atman* (the universal world soul) is more prevalent among Eastern philosophers and writers influenced by Eastern mysticism than among metaphysicians or psychologists in the West. During the latter half of the nineteenth century, however, it attained a popular vogue. " Die Geisterwelt ist nicht verschlossen " became a favourite aphorism. After writing his *Principles*[2] James got increasingly interested in spiritualistic phenomena, and seems to have borrowed both the conception and many of his explanatory metaphors from F. W. H. Myers' *Human Personality and its Survival.* Myers himself speaks of " our individual consciousness " as " but the phosphorescent ripple on an

[1] W. James, *Human Immortality*, 1898; H. Bergson, *Evolution Créatrice*, 1907.

[2] In the *Principles* (pp. 145–182) James has a chapter in which he severely criticizes ' The Mind-Stuff Theory ' particularly in the form of the ' mind-dust conception ' which an evolutionary psychology (he says) apparently demands. He admits that this version is " irresistibly attractive to the intellect "; yet closer examination shows that it is " both logically unintelligible and empirically false ". Empirically, direct observation should convince us that a sensory continuum, like the field of vision, cannot be compounded of separate sensations, as the ' atomistic psychologists' had regularly assumed; it is given to us from the outset in the form of an unbroken whole. Logically the theory is unintelligible, because " all combinations that we know are *effects* wrought by the units *on some entity other than* themselves " (in the original these italicized phrases are capitalized); and this in turn implies some psychic ' medium ' on which the manifold brain-processes can combine their effects. " I confess therefore " (he concludes) " that to posit a soul seems to me the line of least logical resistance ", though to introduce such a concept into a *scientific textbook* would be to forsake the purely " positivistic and non-metaphysical attitude which empirical science should strictly preserve ".

unbounded ocean ". According to James, what divides " the sea of consciousness " into " separate streams " is just the spatial separation of our individual brains; and only in this sense can consciousness as we know it be fairly described as a " function of the brain ".

To the theory in this form there are several obvious objections.

1. First of all, to suppose with James and Bergson that there is and can be no other form of psychic existence but consciousness as such is to abandon all possibility of devising an intelligible explanation of the peculiarities which conscious processes display. Since consciousness exists only in its overt manifestations, it has no dispositional properties. Hence, psychology, in dealing with the phenomena of consciousness, would remain purely descriptive. The only way of interpreting its special characteristics and its various changes would be to assume that they result from the dispositional properties of the brain. To do so may *look* like a reduction in the number of hypothetical assumptions; actually it increases them; for we still have to assign to the brain an assortment of unique psychical characteristics which are quite out of keeping with its physical characteristics; and, as the history of behaviourism shows only too clearly, the attempt to do so is apt to prove extremely misleading.

2. Consciousness is essentially personal and individual. It is impossible to imagine any kind of consciousness which is not somebody's consciousness, and as such it forms a completely closed and isolated system. In his *Principles* James himself placed the strongest emphasis on this unique peculiarity: " the breaches between your thoughts, my thoughts, and everyone else's thoughts are the most absolute breaches in nature: irreducible pluralism is the law ".[1] There was, however, about that time a growing tendency to believe that all forms of discontinuity were delusive appearances in a world where everything was really continuous. The atom itself was depicted as a vortex in a continuous all-embracing medium, the ether. And philosophers and psychologists alike, particularly those with idealistic leanings, were inclined to argue that the discontinuous personal form in which human consciousness appears to manifest itself is also just a superficial phenomenon—the product perhaps of similar vortices in an all-embracing ' psychic ether '. Today, with the almost universal acceptance of the quantum theory, a discontinuous or particulate mode of being no longer seems paradoxical.

3. James' notion that the division of the universal consciousness into separate individual streams might result from its temporary association with individual brains was suggested by the reports published by several contemporary psychiatrists describing how a single personality had, as a result of nervous disorder, apparently been split into two or more different personalities. Already in his *Principles* he had mentioned such cases as being difficult to reconcile with the theory of a personal soul.[2] However, except for three instances of alleged ' possession ' among mediums, all the cases cited turn out to be cases of *alternating* personality, and therefore do not really demonstrate an actual ' splitting ' of consciousness. Almost the only well authenticated instance of ' co-consciousness ' is that of ' Sally Beauchamp ', whose case was studied in great detail by Morton Prince.[3] Later observers have questioned Prince's deductions, and have completely failed to discover any similar cases, such as would confirm his conclusions.[4]

[1] *Principles of Psychology*, 1890, I, p. 226.

[2] *Op. cit.*, I, pp. 349, 379f. Most of the cases are taken from Janet (*L'Automatisme psychique*, 1889).

[3] Morton Prince, *The Dissociation of a Personality* (1906).

[4] Even were such cases definitely established, they could, as McDougall points out, be better explained as instances of 'possession' by a separate pre-existent mind than by a splitting of what was originally a single personality (*Body and Mind*, pp. 370f.; cf. also T. W. Mitchell's discussion of the problem in *Medical Psychology and Psychical Research*). However, most investigators, I fancy, would today maintain that the apparent differences between the various ' personalities ' are only extreme examples of the semi-independent streams of mental activity that form a common feature of everyday life, and are phenomena of attention rather than of consciousness as such.

4. The fact is that James' theory, like all the theories postulating a psychic monism, springs from a confusion of categories. Consciousness, which should be properly described in terms of states and relations, is treated as a kind of stuff or quasi-substance. The history of science is littered with instances of this ' fallacy of reification '. Because things are spatially related, scientists have assumed the existence of a quasi-substance called ' space '; because some things are hot and others are not, they have assumed the existence of a quasi-substance called ' heat ' or ' caloric '; because some things are alive and others not, they have postulated something called ' life ' or ' vital force '. In the end James himself recognized the weakness of his earlier arguments; and in his later essay—' Does Consciousness Exist? '—he abandoned, once and for all, the whole notion of consciousness as a kind of stuff which could be split or compounded.

The Brain as the Instrument of Mind. Many of the objections which have proved fatal to James' version of the theory can be surmounted if we regard the brain as an instrument, not for a universal consciousness, but for an individual mind. By mind I mean, as already explained, not consciousness itself, but a hypothetical entity which is the basis of consciousness in the sense that it can stand in certain conscious relations—it can cognize, and feel, and strive (i.e. seek to attain certain purposes and goals). The simplest analogy that I can suggest to explain what I have in view is that of a machine, in the technical sense in which the word is understood by the mechanical engineer—" a systematic structure of material parts, whose relative motions are strictly constrained and limited, and by means of which the energy at our disposal is transmitted and directed so as to accomplish specific forms of work ".[1] The analogy, like all such analogies, is manifestly imperfect. The brain is not merely a structural but an electrochemical machine. And I should prefer to compare the mind rather to an electrical ' field ' than to a store of ' energy '. But these are side-issues. The essential assumption is that the function of the brain (or certain parts of the brain) is to release and transmit to the material world certain psychic activities of which a mind is capable. In so doing it selects those which are helpful for problems of terrestrial existence, and excludes those which are inessential for the purpose of the moment. The degree of selection and exclusion varies inversely with the complexity of the brain.

Objections to the Instrumental Theory. The most obvious objections to the theory—at least in its simpler forms—have been summarized by Professor Broad.[2]

1. His chief objection is based on reports of brain injuries and their effects on what are ordinarily supposed to be mental powers or mental characteristics. In some cases, for example, the patient seems to lose his power to remember certain events, while his other capacities remain intact. It would hardly be plausible to contend " that all such patients are lying ", i.e., that they could recall the events if they wanted to; " and if ", says Broad, " we reject this violent alternative,[3] we must hold that the injury does actually deprive the mind of its power to remember. Could a supporter of the instrumental theory (he asks) square such facts with his view? " In a rather different type of case the patient before the injury was " of a cheerful and benevolent disposition, and after it he became morose and liable to homicidal attacks: are we to say that the injury has made no difference to his mind?" But, as we shall see in a moment, there are many simple ways in which the instrumentalist can interpret such symptoms, without sacrificing his central hypothesis.

[1] A. B. W. Kennedy, *The Mechanics of Machinery*, p. 2.

[2] *The Mind and its Place in Nature* (4th ed.), 1947, pp. 533f. Cf. also *The Philosophy of C. D. Broad*, 1959, pp. 400f.

[3] There are, however, well established cases of so-called hysterical amnesia, where the patients —from shock or other emotional disturbance—are unable to recall memories; and yet (as hypnosis or free association may demonstrate) the memories have not in fact been destroyed. But in the cases referred to in the text the subsidiary symptoms were quite different from what they would be in purely functional disorders.

2. I should have thought a stronger objection could be based on the fact that direct stimulation of the sense-organs or the brain causes quasi-hallucinatory sensations—e.g., the coloured spot discernible in the visual field when one presses the side of the eyeball through the eyelid, the after-sensations produced by brilliant lights, the vivid sensory experiences and memories evoked by applying electrodes to the bared brain-cortex of patients who have not been anaesthetized. But after all, any instrument, however efficiently it works under normal conditions, may be expected to produce erratic results when the conditions are exceptional and highly abnormal.

Broad's Compound Theory. To meet these difficulties Broad himself puts forward a " modification of the instrumental theory " which he calls the ' Compound Theory '. He suggests that " what we know as a ' mind ' may be a compound of two factors, neither of which separately has the properties of a mind—just as salt is a compound of two substances neither of which by itself has the properties of salt ". He terms them the ' bodily factor ' and the ' psychic factor ' respectively; and the latter he compares to a chemical element or radical which has never been isolated. The ' psychic factor ', he believes, must be capable of persisting after the ' bodily factor ' has been destroyed, and of " carrying traces of experiences which happened to the ' mind ' of which it was formerly a constituent "; but without the collaboration of a body it would manifest neither the higher nor the lower characteristics of a ' mind ', i.e., neither thought nor sentience. " It seems a mistake," he says, " to ascribe perception, reasoning, anger, etc., to a mere body; yet it is almost equally difficult to ascribe them to what is left when the bodily factor is ignored." This joint hypothesis, he believes, would overcome most of the difficulties encountered by both the materialist and the interactionist theories in the forms hitherto propounded. Further, he thinks that there are many phenomena now apparently established by ' psychical research ' —notably the phenomena of mediumship and post-mortem apparitions—which point to some such conception.[1]

Discussing the theory in a later publication, he points out that it might be developed along one or other of three possible lines. (i) We might assume that there is only *one* psychic factor for all minds. This version, he observes, would in many ways resemble T. H. Green's theory of the one Eternal Consciousness and the many animal organisms. But, so far as I can see, this form of theory differs but little from the doctrine of psychic monism proposed by Bergson and James. (ii) Secondly (and this seems the form that Broad himself prefers) we might assume that there is at any one time a different psychic factor for every mind, and that the factor can exist only in combination with a body, though it might pass in immediate succession from one body to another. This, as he points out, in some ways resembles the situation in chemistry where certain radicals, like NH_4 and CH_3, have at most only a transitory existence independent of a compound. Such a version " has advantages for those who favour the theory of metempsychosis " (McTaggart, for example, and many Hindu metaphysicians)—a theory which Broad maintains should be taken far more seriously by Western philosophers.[2] (iii) Thirdly, he thinks we might perhaps consider a theory which combines features from both the foregoing suggestions, and assume that highly individual minds like our own might require each its own psychic factor, but that lower organisms, among which the individual differences are presumably trivial, share a single psychic factor: for instance " it might be held that there is but one factor for the whole species of earwigs ".

In all three versions the basic assumption, it will be noted, is the same, namely, that " mentality is an emergent characteristic of a compound composed of a living brain and something else which is capable of persisting after death and of then entering into temporary combination with the brain of a ' medium '. . . . Such a theory is compatible with all the facts that are generally admitted; and it has nothing against it except a superstitious objec-

[1] *Op. Cit.*, pp. 535f., 651f.

[2] C. D. Broad, *Examination of McTaggart's Philosophy*, 1933, II, p. 639.

tion to dualism ". I should certainly prefer Broad's theory in its second form to that of James and Bergson. Otherwise I see little or no advantage in the more distinctive modifications which he suggests. Indeed they seem open to the following rather obvious criticisms.

(i) To begin with, if we may judge from the few characteristics of the ' psychic factor ' that Broad himself enumerates, it would appear that the factor can lay hardly any claim to the epithet ' psychic '. It is neither rational nor sentient: it cannot therefore of itself be conscious or aware of anything.[1] Broad himself even admits that, though it " produces no ordinary physical and chemical effects, there is no reason why it should not be *material* ". Possibly this is why in his later book (*loc. cit.*, p. 604) he refers to it as ' psychogenic ' rather than ' psychic '. But, so far as his own account goes, it would be just as reasonable to apply this label to the organism itself: actually it would appear to be the two together that are ' psychogenic '.

(ii) Apart from these negative characteristics we are told practically nothing about the intrinsic nature of the ' factor '. Thus, as another critic[2] has urged, it remains " a mere X ", and is no more than " a reified description of the facts which it is postulated to explain ". It supplies no basis for predicting further observable facts which might serve to verify it, and appears to possess no essential qualities of its own.

(iii) Professor Broad is almost equally vague about the psychic compound which the two components jointly generate—namely, " mentality or what we know as the ' mind ' ". If, as he seems to imply, the compound is postulated primarily to perform the functions assigned by other philosophers to the ' pure Ego ', i.e., to act as the subject-term to the various cognitive relations, then it is not easy to see why a ' Mixed ' or ' Compound Factor ' should be able to achieve this more effectively than a ' Simple Factor '.

Parallelism and Panpsychism. In the earlier discussions of the mind-and-body problem attempts were made to avoid the difficulties of materialistic theories on the one hand and animistic theories on the other by adopting a third more or less non-committal hypothesis. " The formula used for this purpose," says Stout[3] (its most influential champion), " is that of psychophysical parallelism." To every conscious state there corresponds a brain-state, and vice versa. " The chain of psychical causation runs parallel with the chain of material causation, but the two do not interact." If any further explanation of this ' pre-established harmony ' is required, we must, he says, fall back not on a scientific but on a metaphysical hypothesis, namely, one which insists on an " idealistic view of all material phenomena ". To the parallelistic formula as thus stated the most obvious answer is that not only are many brain-states unaccompanied by any corresponding conscious state, but also, as we have seen, there are more numerous conscious processes which do not have, and apparently could not have, any analogue in the chain of physical processes. For the rest, nearly all the arguments which weigh against epiphenomenalism weigh equally against this form of psychoneural parallelism.

The doctrine of parallelism, however, can be put in a wider and in some ways more attractive shape. Of the many versions of this broader theory probably the most plausible is that of C. A. Strong. In a volume with the title *Why the Mind has a Body?* (1903)

[1] Broad at times seems a little hesitant on this point: " it remains possible ", he says, " that it *may* have some of the lower factors of mentality, such as sentience; but I do not see anything in the facts to suggest this hypothesis " (*op. cit.*, p. 651).

[2] Professor C. J. Ducasse *ap.* F. A. Schilpp, *The Philosophy of C. D. Broad*, 1959, p. 403.

[3] *Manual of Psychology*, 1904, pp. 50–57. In the latest edition of this work (1938) revised in collaboration with the author by Professor Mace, strong arguments are urged against the doctrine of parallelism as previously formulated; and, in a new section, the doctrine of interaction is treated far more sympathetically.

Strong rejects the ' instrumentalist's reply ' as formulated by James and Bergson, and puts forward a doctrine of panpsychism which he has since worked out in considerable detail.[1]

Strong's ultimate aim is to provide a theory which will be compatible with a modified metaphysical doctrine, which he calls ' critical realism '.[2] The older school of ' new realists ', who had risen in revolt against the ancient idealistic creeds, maintained that the sense-data, which are ' given ' in ordinary sense-experience and are commonly regarded as ' contents of consciousness ', are in fact components of the physical world: grass is really green, tomatoes are really red. To this Strong rightly replies that, if ' givenness ' is an intrinsic character of the physical objects we perceive in sensation, it would be difficult to account for the 'non-givenness' of those physical objects which nobody perceives. To call in a *deus ex machina* like Berkeley's ' omniscient all-perceiving deity ' would be no genuine solution. ' Givenness ' therefore must be the essential characteristic of a non-physical ' vehicle ' which he terms a ' psychic state '. The ' self ' is made up of such ' psychic states ', and must be considered " as a substantive rather than a separate substance ". The self or ' psyche ' thus consists of ' mind stuff ', and " mind stuff exists in space as well as in time ". Moreover it has no unity beyond the unity of the brain. The apparent paradoxes involved in this somewhat complicated theory are overcome by invoking the ancient ' double-aspect ' doctrine: all reality has both a physical and a mental aspect. Thus " the body is the appearance of the psyche to the outer senses "; and the psyche itself is " at once psychic and extended ".

Except in its rejection of instrumentalism in the narrower sense this theory closely resembles that of James. Like James, Strong believes in an all-embracing sea of mind stuff, which is apparently subdivided into separate individual ' psyches ' solely because of its association with separate individual brains. Evidently most of the criticisms we have against James also hold against Strong. To examine the merits of the various metaphysical doctrines used to eke out the rival psychological hypotheses—idealism, realism, pragmatism or the like—falls outside our present scope.

IV. Summary and Conclusions

A Suggested Model. By way of summing up the results of the foregoing arguments let me now attempt to put together my own conception of a ' mind '.

(1) First of all, I regard it as a ' continuant ' in Johnson's sense: it is not *merely* an ' existent ' or set of existents (for that would allow us to treat it as no more than a complex event or system of events); it is a particular type of existent, characterized by a certain degree of permanence. And in that rather restricted sense I should be prepared to describe the mind as a ' substance '. This first qualification rules out the conclusion which James reaches at the end of his long discussion about the ' stream of consciousness ' and the ' nature of the self ', namely, that " if the *passing thought* be the directly verifiable existent, then that thought is itself the thinker, and psychology need not look beyond " (*Principles*, I, p. 401).

(2) Secondly, the mind possesses a certain degree of *unity and independence*. It must in my view be a unity, because it is to serve as the basis of the individual's personal consciousness, and that, as we have seen, has unity as one of its most distinctive characteristics. It is independent both of other minds (i.e. of the bases of other persons' consciousness) and of the physical organism with which it is associated. I fully agree that the evidence provided by ' cross-correspondences ', and still more by reports of alleged apparitions after death, is

[1] In *The Origin of Consciousness*, in the later *Essays on Natural Origin of the Mind*, and finally in an important article in *Mind* (XL, 1931, pp. 217f.). It should be added that Fechner, as every student is aware, advanced a doctrine of psychophysical parallelism which included both the forms discussed and endeavoured to support it by reference to his famous law relating psychical sensation to physical stimulus.

[2] *Essays in Critical Realism*, 1920, pp. 225f.

far from convincing. Nevertheless, I hold with Broad that there is perhaps just sufficient to make it slightly more probable than not that something ' psychic ' may survive after death, and continue to exist independently of any material body or brain. To many no doubt the issue will seem a matter for faith rather than for scientific proof. In any case it is important enough to forbid us at the very outset staking everything on a theory which would completely rule out any such possibility.

(3) Thirdly, the mind, like other ' substances ', has certain *dispositional properties*. Of these, the most distinctive—the *differentia*, as the older logicians would have said—is the capacity for consciousness in the narrower sense of the term, i.e., for cognitive ' awareness .' It is this third qualification that makes the ' factor ' or ' entity ' which we are postulating specifically ' psychic '. ' Awareness,' as I have argued elsewhere, is a simple and unanalysable dyadic ' relation ' : as such it requires not only an ' object ' (i.e. a sensum or an image) but also a ' subject '. Or, to adopt Broad's way of putting things, it involves an ' act ' of prehension; and an act of prehension implies an agent which does the prehending. The ' mental event ' is simply this ' act ': if mental events did not happen to mental substances (as physical events happen to physical substances), then, as he observes, no event could consist in the prehension of anything.

But I hold that there are other types of cognitive relation besides ' awareness ': cognition in my view comprises not only (i) ' awareness ' or direct apprehension of (*a*) sensa and (*b*) images; it also includes (ii) intuitive forms of indirect (or referential) cognition, as in (*a*) perception and (*b*) memory, and (iii) discursive forms of indirect cognition, as in (*a*) judgement and (*b*) reasoning.[1] None of these forms of cognitive relation—the higher forms, as they are sometimes termed—is reducible solely to one of the lower or simpler relations. Together they guide or steer the activities of the embodied mind; they are, in current jargon, ' cybernetic '. But I also hold that consciousness in the broad sense includes other relations besides those that are strictly cognitive. It includes certain affective or evaluative relations (being pleased with an object, etc.). Hence among the various dispositional properties of the mind I should place a capacity to appreciate values (and this in my view forms one of the most important objective characteristics of the universe we live in).[2] This property enables us to accept the everyday assumption of personal responsibility, and provides another striking peculiarity which distinguishes a mind from a mere material substance. Finally I hold that consciousness includes the possibility of certain conative relations. These last two sets of relations form the basis for the familiar theories about orectic or hormic ' energy '. Thus the mind as I conceive it includes not only a system of *directive* dispositions but also a system of *dynamic* dispositions.[3] The nature of these 'dispositional properties', and of others equally distinctive but more specific, will be expressed in terms of what are sometimes called ' scientific laws ', i.e., statements about general tendencies of the form " *if* so-and-so, *then* so-and-so ". These laws will plainly be quite different from the laws describing the dispositional properties of material substances, and could not possibly be derived or deduced from them. It is the business of psychology to discover and formulate such laws.

[1] The evidence for this is not merely introspective but statistical: see Burt, ' The Structure of the Mind : A Review of the Results of Factor Analysis, ' *Brit. J. educ. Psychol.*, XIX, 1949, pp. 101–111, 176–199.

[2] Mr. Gregson and others (see this *Journal*, XIV, pp. 72–75 and refs.), have challenged this assumption on the ground that ' value ' is not an objective quality and certainly not a concept that is permissible in a natural science. In reply I will only say that I accept as completely convincing the arguments brought forward by Professor A. E. Taylor to show that " the supposed absolute disjunction of fact and value is merely a mistaken prejudice " (see his Gifford Lectures on *The Faith of a Moralist*, pp. 46f.). Without such an assumption a psychology of delinquency and crime would become impossible.

[3] See Burt, ' The Factorial Study of the Emotions ', ap. M. L. Reymert, *Feelings and Emotions*, 1950, pp. 531–551.

(4) It is evident from this brief review of its various properties that a mind, so far from being a ' *simple* substance ' must have a distinctive *structure*. Further, I believe that each individual mind will not only exhibit the general structure common to all minds, but will also develop structural peculiarities of its own. This would imply that the mind possesses a disposition to acquire dispositions, i.e. to form what would ordinarily be called memories and character-traits.[1] Could this conclusion be empirically established, we should then avoid two incidental difficulties: first, that a mind endowed solely with general properties would, if it survived, be wholly impersonal; secondly, that, if personal minds are sufficiently permanent to survive after death, they must for the same reason have existed before birth—a notion which seems to me far less plausible than it does to Broad and McTaggart. Indeed, since scientists are nowadays prepared to contemplate as a tenable hypothesis the continual creation of atoms (as the Hoyle and Bondi ' steady state ' cosmology requires), there can be no reason why we should not also imagine—still merely as a hypothesis—the continual creation of minds. At some stage before birth we should then have to suppose that an individual mind emerged, as it were, first in the form of what Heisenberg has called a ' potentiality ', i.e., as a ' mere seed, germ, or bud '; and personality would be something that gradually unfolds and develops during terrestrial life.

There are at least three ways in which the foregoing conception differs from the Cartesian theory, as Dr. Smythies and Mr. Hudson represent it. According to that theory, " the mind or self is not only a simple substance, it is also an unextended substance, and is something of which in introspection I am directly aware ". None of these features forms part of my definition.

(i) To begin with, the mind as I have described it is something complex, and it has a history. If so, I can no more be ' directly aware ' of my ' mind or self ' than I can of any physical object: the most I could directly apprehend would be the thinker or perceiver of the passing moment. Many psychologists indeed would hold with Hume that they have no knowledge whatever of any such thing as a self. After all, awareness is from its very nature an asymmetrical relation; and should we not agree with the Hindu philosopher: " The eye sees not itself, the ear hears not itself, and the knower knows not himself ". In reply I should argue that it is the mind that sees and hears, not the sense organ; and, just as I can legitimately talk of loving myself or being pleased with myself, so, in a certain sense, I may legitimately talk of knowing myself. There is no reason why an asymmetrical relation should not also be reflexive. Yet even so, I readily admit, my knowledge of myself must be more like my perceptual knowledge of a material thing—the clock upon my mantelpiece, for instance—than a direct or intuitive awareness of a permanent ego.[2]

[1] See the experimental work reported by McDougall and May Smith, *Brit. J. Psychol.*, X 1920, pp. 143f.; also *Body and Mind*, pp. 334f. Here, however, I must confess, the evidence so far available appears to my mind decidedly weak: it is quite conceivable that the special structural characteristics acquired by the individual may be merely acquired structural modifications of the individual's brain.

[2] Descartes' own account certainly seems quite unconvincing. " Of every substance," he says, " there is one principal property which constitutes its nature or essence. Thus extension in length, breadth, and depth constitutes the nature of corporal substance, and thought the nature of thinking substance " *Principles of Philosophy*, sect. LIII). In sect. XXXII he says: " there are only two modes of thinking, viz., the perception of the understanding ", which includes " to perceive by senses " as well as " to imagine and to conceive " and the action of the will. However, in the *Meditations* perception and thought are contrasted. Perception, we are told, is commonly thought to be " an attribute of the soul " like nutrition and locomotion (the reference is to the Aristotelian conception of the *psyche* as adopted by the scholastics; nevertheless, " perception, like walking or taking nourishment, is impossible without a body " but thinking does not need a body. " Here therefore I discover what properly belongs to myself: I am, properly and precisely speaking, only a thinking thing, that is a mind (*mens sive animus*)." Animals do not think; therefore they have no minds.

(ii) The Cartesian assumption that the mind is a substance with neither location nor extension in space apparently forms the chief reason why so many psychologists reject the concept of mind in favour of a conscious brain which possesses both location and extension. With Russell and his disciples they maintain that my consciousness is ' in my head '. This seems rather like debating whether or not gravitation is ' in the earth '. I suspect that the psychologists in question picture the brain as a *tangible* thing in *perceptual* space. But the physical objects of science are not in perceptual space but in physical space or rather space-time; and for the physicist space or space-time is not an observable container in which things are visibly extended, but merely a name for a particular system of relations. Hence, much as I dislike using an ambiguous preposition like ' in ' of mind or consciousness, I see no reason why the mind should not be ' in space ' (in the physicists' sense) or ' extended ' (as I suppose a field of force is ' extended '). But the issue seems largely verbal and little more than a gratuitous pseudo-problem.

Interactions. The dispositional properties of the mind, like those of other substances, will be displayed by its interactions. The interactions of purely physical substances may be described as ' physico-physical '; and include both inter-physical processes (such as the interaction of an acid with a metal, or a proton with an electron) and intra-physical processes (as in beta-decay, or in growth and senescence, though most intra-physical reactions could probably be reduced to inter-physical reactions between the constituents of various compounds). Nearly all physiologists postulate interactions that may be described as physico-psychical, like those involved in sense-perception; but they apparently hesitate to accept the notion of psycho-physical or psycho-kinetic interactions, such as most laymen believe to occur in the case of volition. Both these types, however, are essential to my theory. In addition I assume at least the *possibility* of inter-psychical reactions, as in telepathy, and of intra-psychical reactions, as in reasoning (when it forms the ground for choice) and possibly in memory (as in the mode of recall postulated by Russell's theory of ' mnemic causation '). A strong confirmation of my general hypothesis would therefore be provided if a series of experimental or statistical investigations could be planned such as would yield positive evidence in favour of these additional types of interaction.

The Mind as a Field. To psychologists brought up, as so many of us still were, on the elementary notions of classical physics, the idea of an immaterial something, endowed with dispositional properties having no physical basis for their support, must sound almost as incredible as the idea of an immaterial something, endowed with electrical properties, would have seemed to the physicist at the outset of the nineteenth century. Electricity they thought of as a material fluid flowing through a physical wire. Maxwell pictured Faraday's electrical and magnetic field as a system of stresses and strains in the " contiguous particles of a universal medium ". It is only since Einstein " finally abolished the aether " that physicists have grown accustomed to the notion of a ' field ' without supposing that there must be some underlying physical medium to explain it.[1]

[1] In its simplest form the physicist's notion of a ' field ' may be explained by considering the case of the gravitational field. At any given point the attraction which results from a given distribution of mass can be expressed in terms either of a force which would act on a unit point mass placed there, or of a gravitational potential existing at that point and satisfying certain differential equations. The potential is literally ' potential ' in the sense that it does not describe a material property of the field, but may have a value in empty space: it thus describes a *dispositional property*, namely, the force that *would* be exerted *if* a unit point mass were introduced into the field. In this sense, as Hertz observes, " the field-strength is a reality which exists even when the reacting bodies are removed ". Planck uses almost exactly the same phrase: (*Theory of Electricity and Magnetism*, 1932, p. 110); and Reichenbach applies the word ' substance ' to describe the field (*Atom and Cosmos*, 1932, pp. 124, 187f.). Note that the force has a *direction* as well as a *strength*; (every schoolboy is familiar with the experiment in which, say, the north pole of a magnetized needle transfixed vertically through a floating cork, moves in a definite direction along a curved path away from the north pole of a bar magnet around towards the south pole).

Now if we think of the dispositional properties of the mind as psychic ' tendencies ', analogous to the physicist's ' potentials ' or ' forces ', we can regard the mind as a kind of ' field ' existing in the neighbourhood of an individual brain, much as the physicist conceives of an electromagnetic field existing in the neighbourhood of a set of electric currents. The tendencies would exhibit both dynamic and directive characteristics. Such a field would of course differ from the more familiar types, just as electromagnetic fields differ from gravitational, topological from metricized, and quantized from unquantized. But the scientist of today shows no hesitation over postulating new types of field when they seem needed to interpret phenomena which would otherwise escape unexplained.[1]

The Double-Filter Theory. A plausible hypothesis, not unlike the so-called ' instrumental theory ' of James and Bergson, might, I think, be developed, if we consider the biological implications of a classification of cosmic processes which has often been involved in discussing the newer physical theories. We can divide the whole scale of spatial and temporal quantities with which the different sciences deal into three broad orders of magnitude. We can then distinguish between what I will call macrophysical, mesophysical, and microphysical phenomena respectively.[2] By mesophysical phenomena I mean those in which the spatial dimensions, the time-intervals, and the velocities do not greatly exceed or fall short of those we encounter in our everyday perceptual experience. They are the familiar phenomena which are regularly described in the language of ordinary life and in terms of naïve realism. Here too belong not only the problems, the concepts, and the models of classical physics, but also (be it noted) the observations made in every attempt to verify a physical hypothesis in the laboratory or elsewhere, no matter which class of phenomena is under investigation. By microphysical phenomena I mean processes below this range, particularly those of the atom, the atomic nucleus, and the various elementary particles. At this level entirely new concepts and models are necessary—those associated with quantum theory. By macrophysical phenomena I mean phenomena in which the spatial dimensions and the time-intervals are literally of astronomical dimensions, and the velocities so high as to approach the speed of light. Here belong the novel concepts and problems of cosmology, and more particularly those dealt with in the theory of relativity.

In modern investigations of microphysical and macrophysical phenomena it has proved extremely difficult to ignore the interactions of the conscious observer with the system under investigation. Consequently the borderline between physics and metaphysics tends to disappear,[3] and both Occam's rule of parsimony and the postulate of simplicity are apt

[1] Not only telepathy (if it exists) but also every form of cognition is essentially a mode of action at a distance. Now in physics a common reason for postulating a ' field ' is to furnish a convenient model for interpreting action at a distance: and in my view this is one of the many advantages of adopting a similar model in interpreting the phenomena of consciousness.

[2] The first to emphasize and label the basic distinction seems to have been Eddington: he distinguishes what he calls ' macroscopic systems ' which " broadly speaking are covered by relativity theory " and " microscopic systems covered by quantum theory ". The term, however, is not altogether appropriate, since the 'entities' which he specifies—protons, electrons, photons, etc.—are submicroscopic. The term ' macroscopic ' includes what I have called ' mesophysical '.

[3] Von Neumann's brilliant examination of the process of quantum mechanical measurement plainly demonstrates that " the laws of quantum mechanics, with its various implications, cannot be formulated without recourse to the concept of consciousness " (J. von Neumann, *Mathematische Grundlagen der Quantenmechanik*, Engl. Tr., 1955). Heisenberg puts the situation very clearly. " The mechanics of Newton, and of classical physics constructed after its model, started from the assumption that one can describe the world without speaking about ourselves. This seemed almost a necessary condition for natural science. But the situation changed as a result of quantum theory. . . . The system which is treated by the methods of quantum mechanics remains part of a much bigger system: it could not in fact belong to the world of observable phenomena if it had no interactions with this bigger system of which the observer is a part." Thus, contrary to the assumption of the Newtonian physicist, "natural science does not simply describe and explain nature; *it is*

to break down. Contrary to all expectation the number of basic ' entities ' has steadily increased; and the equations have become more and more complex. The simpler formulae of classical physics turn out to be only working approximations obtained in those intermediate cases where the mesophysical phenomena are sufficiently large for Planck's constant to be neglected and the velocities are sufficiently small for the propagation of light to be treated as instantaneous. Thus " the rigorous laws of physical science " to which Watson, Hansel, and others would like psychology to conform are no more than approximate probabilistic laws.

Now the terrestrial world in which the human mind as we know it has presumably evolved is essentially a world of mesophysical dimensions. In order to survive in such a world the size of man's body cannot exceed or fall short of certain limits, and the range and speed of his movements are correspondingly restricted. A sensory awareness of phenomena greatly beyond the limits thus imposed would be a hindrance not a help. As a result the brain and sense-organs with which natural selection has equipped him are highly selective. The potential stimuli arising in his terrestrial environment are thereby filtered and sifted before they are allowed to enter his mind. Radiations exist well beyond both ends of the visible spectrum: the ' waves ' used for wireless transmission are 500 yards or more in length; X-rays are only about 1/10,000 inch in length; and gamma rays and cosmic rays less than 1/200,000,000 inch. If the eye could see the whole range of these invisible colours, the spectacle presented by the universe would be far richer and far more informative than it actually is. But with X-rays objects would seem transparent. Rays of a wavelength much longer than light (heat rays, for example) turn corners, and would be of little use for direction-finding. For these and other reasons human vision is therefore limited to a narrow band between 16/100,000 and 32/100,000 inch. Man's chemical senses are poorer still; and he has no sense-organs that enable him to recognize electrical or magnetic fields. Nature allows him to perceive only those properties of the material world which are really important for his survival as a land animal—sustaining and defending himself and his family on a very restricted portion of a very unusual planet; a glance at the vast number of instruments carried on an aeroplane and the innumerable precautions taken to protect an astronaut, is sufficient to demonstrate how scantily the human body is equipped by nature for appreciating conditions other than those for which it was actually evolved.

Man's interpretation of the information which his senses supply has also to be limited to the barest essentials. For quick and efficient action the universe must appear extremely simple, and man must think of it in simple ways. Day-to-day life must seem so plain and straightforward that " the wayfaring man, though fool, cannot err therein ". Much of his perceptual interpretation his brain automatically supplies of itself. And, were he forced to recognize how incomplete and subjective his picture of life really is, he would hesitate over every action: a race of reflective Hamlets would become extinct with the very first generation. Consciousness itself therefore has to be as transparent as a glass window, so that the man of common sense, like the behaviourist, can ignore its very existence.

The brain, then, in transmitting information about the cosmos to the conscious mind, must permit only what is absolutely indispensable to percolate through. But equally, I believe—though here we are on still more speculative ground—the brain must also impose restrictions on what the conscious mind itself is capable of performing. Faced with the problems of survival or of advancement for himself and his family, the practical man,

itself an interplay between nature and ourselves " (W. Heisenberg, *Physics and Philosophy: the Revolution in Modern Science*, 1959, pp. 74–5; cf. pp. 153–4). As he sums it up elsewhere, " the conception of objective reality has evaporated into the transparent clarity of a mathematics that represents no longer the actual behaviour of elementary particles, but rather our *knowledge* of this behaviour " (*Daedalus*, LXXXVII, 1958, p. 100).

primitive or civilized, seems compelled to think first—and often solely—of the significance of things in that very limited context: they are viewed as means, never as ends in themselves.

The poet and the artist, on the other hand, sees or claims to see them as possessing a significance of their own. For them " the meanest flower that blows . . . meadow, grove and stream, the earth and every common object ", have each its own peculiar value, and appear, not merely ' apparelled in celestial light ', but glowing (so to speak) with an intrinsic meaning and a glamour of their own. At the same time, however, " it is his absorption in what he takes to be these deeper meanings that makes the artist seem at times abstracted and unpractical—an absent-minded dreamer lost in ethereal visions ".[1] Similarly the abstract scientist and the philosopher, who pursue truth for its own sake rather than for its practical benefits, have often appeared figures of fun to the man of common sense: Thales, gazing at the stars, falls into the well. But the supreme example of this contrast between the search for supposed intrinsic value and the pursuit of material satisfactions is the mystic. On a strictly materialistic view, such as Dr. Bolton advocates, we are bound to regard all mystical experiences as " the illusory effects of a semi-pathological condition ", and hence to reject their implications. And yet, when we consider how much modern civilization, and even science itself, owes to such people—to Plato and Plotinus, to Buddha and the anonymous Deutero-Isaiah, to Paul of Tarsus and to Augustine of Hippo—is it not a little rash to be so confident of the present stage of human knowledge that we can wholly dismiss what they claim to have discovered from their wider range of consciousness? Without necessarily committing himself, the psychologist must at least leave room for the *possibility* that their claims may be in some measure justified. Indeed, as William James maintained in his studies of such states, " the conclusion seems often forced on us . . . that our normal, waking, rational consciousness is only one rather limited type: round about it, parted by the flimsiest of screens, there lie potential forms of consciousness entirely different. We may go through life without suspecting their existence; yet no account of the universe can be final which leaves them disregarded ".[2]

Ordinarily, in the interests of survival, the brain shuts them out; but in certain more or less abnormal states[3]—states of dissociation, and the like—these latent capacities seem at times to force their way out and become highly active. Such states, however, are exceptional. The material and utilitarian outlook of civilized life almost inevitably forbids any diversion of attention to the remoter planes of cosmic existence. And for these reasons, we may imagine, as Dr. Tyrell has observed, " clairvoyance, telepathy, and other paranormal phenomena, like those of religious experience and mysticism, crop up far more frequently among primitive tribes and individuals, and seem all but extinct among the highly civilized, the highly commercialized, and the scientifically trained ".

Conclusion. Briefly then the hypothesis I suggest may be summed up as follows. The mind is a unique type of ' continuant ', characterized by a certain unity and independence, and endowed with certain dispositional properties and a definite structure, which are essentially psychic. It thus provides a relatively permanent basis for consciousness. It is not to be interpreted as a resuscitation of the ' simple, unextended, indestructible substance ' of Cartesian metaphysics. It resembles, not the traditional *anima* or soul, but rather what the physicist would call a ' field '. In the end research may possibly show that the ' continuant ' has no greater permanence than the individual's terrestrial life—perhaps that it could not even come into existence apart from a special kind of physico-chemical sub-

[1] I quote from my chapter on the ' Psychology of Art ' in *How the Mind Works*, where I have discussed this view of ' value ' at some length.

[2] W. James, *Varieties of Religious Experience*, 1904, p. 427.

[3] Nevertheless, an abnormal state is not a *sine qua non*. John Buchan, for example, records that, as a rule, he had these rare quasi-mystical experiences with their ' wonderful clearness of mind ' and the ' sudden relevation as in poetry or music ', when he was ' very fit in body ' (*Memory-hold-the-door*, 1940, pp. 120–21).

structure—a living organism or brain. At the moment, however, the wavering balance of evidence seems to point rather in the opposite direction. But the main advantage of such a conception, as compared with the purely physiological interpretations favoured by materialists, physicalists, and behaviourists, springs from the fact that it does not from the very outset preclude investigations concerned with conscious processes as such or with the alleged paranormal phenomena studied in psychical research; and it furnishes us with a far more convenient framework for describing the conscious processes of everyday life, and consequently for the various branches of applied psychology. Finally, let me repeat, the whole conception is presented, not as a theory fully confirmed by *a posteriori* evidence, but merely as a tentative model, suggested by certain recurrent phenomena, implying certain testable corollaries, and urgently needing to be verified, refuted, or modified by *ad hoc* empirical research.

HIGH ABILITY AND HIGH ACHIEVEMENT

To the Editor, British Journal of Statistical Psychology

SIR,—In his recent discussion of social mobility Professor Burt appears to assume that his assessments of ' intelligence ' are measurements of an intellectual capacity that is purely, or at any rate mainly, innate. I am aware that in previous articles he has already carried out an elaborate analysis of variance in the endeavour to show that, in theory, intelligence is essentially an inborn characteristic. But, in discussing such phenomena as social mobility, we are concerned not with psychological theory, but with the practical performance of actual individuals, and of individuals of particular types—the mentally subnormal and the mentally supernormal, whether or not (as we are asked to assume) they inherit their subnormality or supernormality from their subnormal or supernormal parents.

The educational system Professor Burt would have us establish implies a society in which class distinctions, instead of being reduced or superseded, are to be sharply accentuated. He displays a surprising faith in the predictive value of examinations and tests, and appears to claim for the ' gifted groups ' discovered by their means a complete freedom from almost every type of blemish. As to the outcome, were such selective methods to achieve success, two views are possible. According to his view, they would result in a community arranged in a hierarchy of *intellectual* grades; presumably with a glorified public school for the intellectual élite at the top—in short, the rise of meritocracy. According to the view accepted by most contemporary psychologists and sociologists in America as well as in this country, they would create or perpetuate a community arranged in *social* grades, based on income, privilege, or sheer intellectual snobbery.

Since, as we all agree, ' gifted ' pupils are commonly the offspring of ' gifted ' parents, it is after all only to be expected that we should find them chiefly in professional classes (or the equivalent ranks in commerce or industry). In these upper social classes the parents are obviously in a position to supply their offspring with far richer opportunities both at home and at school for developing ' gifts '. Giftedness is thus not a biological endowment but the superficial effect of the unfair advantages that accrue to members of the privileged and well-to-do classes. Galton's own collection of pedigrees and family histories are far from proving his hereditarian thesis. All they really demonstrate is that the special achievements of men of genius are predominantly the result of the special facilities and the special stimulation afforded by their environment. I suppose the most obvious hall-mark of intellectual achievement is a Fellowship of the Royal Society. Yet consider how many of them—from the first President, Lord Brouncker, and the Honourable Robert Boyle, ' father of chemistry and uncle to the Earl of Cork ' down to quite recent times—have been noblemen, or members of wealthy and aristocratic families, or close friends of the Court like Pepys and Evelyn. Surely the work of Dr. Fleming, Dr. Heim, Dr. Floud and others has abundantly shown that the role formerly attributed to heredity by Pearson, Terman, and other members of the Galton school was greatly exaggerated, and that individual differences in ability or aptitude are due far more to social and environmental influences than to genetic conditions.

A. L. HARRISON

SIR,—In a recent article (this *Journal*, XIV, 1961, p. 13) Professor Burt cites figures showing that nearly half the children of high intelligence fail to achieve an occupational status equivalent to their capacity; and in other papers addressed to educationists and teachers (*Brit. J. educ. Stud.* VII, pp. 114f., *Educational Research* II, pp. 241f.) he complains that, in our eagerness to do all we can for the mentally handicapped and the large mass of average children, we have neglected to devote sufficient attention to those who are of outstanding ability. May I suggest that for this psychologists themselves are largely

to blame? Many of them seem to imagine that the importance of environment, and of environmental improvements, can only be brought home if the importance of heredity and genetic conditions generally is belittled or denied. And those who believe in the importance of both fail to present their evidence in a way which will convince the practical worker and the general public. The highly complicated algebraic proofs offered by Fisher, Burt, Howard and others are quite incomprehensible to the average teacher or educationist, and probably I suspect to most psychologists. Would it not be possible to give a few straightforward arguments, based on plain and concrete evidence, without all these mathematical complications?

And why do psychologists persist in treating the problems of the supernormal as if they were of the same general type as the problems of the subnormal? Throughout their discussions both issues seem to be regarded as no more than special aspects of the broader and vaguer problems of ' general intelligence '. Even when they turn from their curves and their calculations to talk about actual individuals, we still hear far more about the subnormal than about the supernormal. We have books on ' the backward child ' and books on the ' mentally deficient ', but no book on the ' gifted child ' as such. It is principally for these reasons that the question has been so widely ignored by the public at large. Meanwhile, the critics of the theories Professor Burt so strongly advocates—the ' general factor ', the genetic contribution, and wide range of innate differences—formulate their objections in everyday, popular language which all of us can understand, blandly ignoring the scientific evidence based on statistical analyses and silently passing over the statistical evidence and the vast array of quantitative data, no matter how thoroughly it has been tabulated, analysed, and reduced to symbolic equations. As a result their views are widely accepted, not because their arguments are conclusive, but merely because they alone are intelligible.

I suggest that, in the present state of psychology, the function of a statistical journal is not merely to appeal to the statistical specialist, but also (if I may use the word) to *popularize* statistical methods and statistical conclusions by recasting them in non-statistical form.

J. F. Somerville

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CRITICAL NOTICE

THE PSYCHOLOGY OF PERCEPTION

The Problems of Perception. By R. J. Hirst. London: George Allen & Unwin; New York : The Macmillan Company, 1959. Pp. 330. 30*s*. net.

A glance at the writings of present-day physicists is sufficient to show how the recent revolutions in physical theory have revived in a sharpened form all the time-honoured problems of perception, and revealed a host of new puzzles and paradoxes crying out for investigation. As Dr. Hirst rightly insists, the study of perception and of what he calls ' perceptual consciousness ' is primarily a task for the psychologist; and yet, as the physicist's interest in the problem has grown, the interest of psychologists has faded and flagged. Fortunately, the current changes in philosophy have helped to keep the problem alive; and it is greatly to be hoped that Dr. Hirst's lucid and critical review may induce psychological investigators to turn their attention once again to a topic which formerly occupied the opening chapters of almost every textbook.

It is true that in the thirties the conflicting theories of post-Watsonian behaviourists stimulated a number of fruitful researches on some of the processes underlying perception but they commonly treated it, not as a unique type of cognition, but as merely a mode of learning or of communication, of verbal report or neural functioning. Perceptual consciousness as such was assumed to fall outside the legitimate pale of scientific inquiry, for the simple reason that all forms of consciousness are purely private phenomena, while science can deal only with what is public.

Statistical psychology, however—at any rate in this country—has been less affected by these psychophobic trends; and I venture to think that the approach which the statistical investigator habitually adopts, and the outlook that he acquires from his somewhat peculiar methodology, may help him to reach a solution which in some ways may prove more satisfactory than any of the conflicting theories advanced hitherto by the philosopher, the physiologist, or the experimental psychologist. They tend to oversimplify. Their theories are framed in terms of mutually exclusive, clear-cut categories. They ask such questions as: are our percepts private or public, objective or subjective? are sensory qualities physical or mental? And their central problem is commonly the problem of causation: are our percepts generated by the physical stimuli, by the neural structure of the brain, or by something called an ' ego ' or a 'mind ', and do they have any causal efficacy of their own? The most important outcome of this mode of approach is ' the causal theory of perception ', which, so Professor Price tells us, has become ' the official foundation of natural science '—of natural science, that is, as understood by physiologists, doctors, and the majority of psychologists.

The statistical psychologist thinks in terms of variations and covariations rather than of causal properties or verbal classification. He sees phenomena as part of a multivariate pattern; and he asks, not which is the one essential factor, but which of the many contributing factors are the more important. But the most vital difference, I fancy, is this. The traditional outlook of the philosopher, and of the school of classical physics in which most of the earlier behaviourists were trained, leads them to seek conclusions that are absolutely sure and certain. If the various forms of perceptual consciousness cannot be operationally defined, or if statements about it cannot be objectively verified, then such notions and such statements must be banished from psychology as a reputable branch of knowledge. Or again, if, as some would hold, sensory phenomena are the only things of

which we can claim infallible knowledge, then, according to a rival school, sensory phenomena are the only things that we can validly assume to exist. Time after time, as we learn from Dr. Hirst, the phrase " such a view is tantamount to denying the possibility of knowledge " is treated as a cogent *reductio ad absurdum*. The statistical psychologist adopts a more modest standpoint. He distrusts all claims to certainty, and is perfectly content to assess probabilities. He presents his own solution, not as an authentic description of what really happens, but merely as a hypothetical model—usually as just one of several possible models, each appropriate for some particular purpose.

With much of this, I suspect, Dr. Hirst would readily agree. In his opening chapters he criticizes the epistemologist for " the attempt to say with certainty what the object of awareness is "; and he deprecates the familiar type of syllogistic argument that takes as its premises " this or that (e.g., a particular type of observable datum or sensible quality) is indubitably present " and then seeks to deduce equally " indubitable conclusions ". Nevertheless, in the body of his work Dr. Hirst himself seems to fall back very largely on armchair deduction, and the stock academic *Gedankenversuche* about speckled hens, elliptical pennies, sticks in the water, and the reflected duplicates that philosophical Alices seem so surprised to discover when they gaze into the looking glass. On the other hand, he makes comparatively little use of findings reached by planned observations or recent experimental researches.

At the outset of his book Dr. Hirst largely follows in the steps of his predecessor, Professor Price.[1] Like him, he begins by considering the difficulties involved in " the usual statement of ' naïve realism ', the alleged creed of plain men ", which (as he very truly remarks) is " not at all fair to the plain man ". He then devotes two closely reasoned chapters to a drastic, but to my mind not wholly convincing, criticism of the doctrine of ' sense data ', as formulated by Moore, Broad, Ayer, and Price himself. From this he turns to the more important contemporary theories—first, that of ' phenomenalism ' as formulated in the later work of Russell and the earlier work of Ayer, secondly, that put forward by the champions of the ' linguistic approach ', notably Ryle, and thirdly, the modern variants of the ' representative theory ', more particularly those developed by neurologists, like Eccles, Russell Brain and J. R. Smythies. Each of these interpretations, he concludes, is open to grave objections, springing chiefly from untenable assumptions about the relations between consciousness and brain.

This leads him to an interesting digression on the problem of ' mind and body '. The interactionist theory is rather summarily dismissed. The behaviourist view as formulated in Ryle's *Concept of Mind*, together with his forthright rejection of introspection as a source of evidence, is subjected to a full and highly critical examination, with the result that Ryle's ' main thesis ' is rejected as ' quite incredible '. In place of it Dr. Hirst propounds what he calls an ' Identity Hypothesis '. " The person," he maintains, " is a unity, not an association of two distinct substances . . . An experience and a pattern of brain process are the same event differently revealed." Thus " the mental and the cerebral activities of the person are really two aspects of an identical activity ".

This version of the ' double-aspect theory ' is supplemented by a ' Sentience Hypothesis '. To justify it Dr. Hirst draws a sharp distinction between " percepta " (i.e., " how the object actually appears to the observer ") and " percipienda " (i.e., " those theoretically apparent properties which, from our knowledge of its nature and position, etc., the object should appear to have "). The discrepancies between the two he attributes to " modifications by the percipient of a basic awareness or ' sentience ' which corresponds to the theoretically apparent properties ". The nature of these various modifications is then investigated in a chapter on ' the psychology of perceptual consciousness '. This I find disappointing. The heading leads one to expect an up-to-date survey of psychological researches, old and new, on the various problems involved. Instead there are one or two

[1] H. H. Price, *Perception* (1932).

incidental references to textbooks by Woodworth, Koffka, Hilgard, and Gibson,[1] a somewhat cursory discussion of ' stereoscopic vision ' and ' figure-ground and constancy ', and an ingenious version of the ' television tube analogy '; and from these a theory of four basic types of modification is derived, viz., (i) the unconscious selection of certain parts of the percipienda through involuntary attention; (ii) a fusing of sentience with imaginative or conceptual elements; (iii) the unconscious use of cues (e.g., for visual depth and object constancy); (iv) various effects produced by learning and experience. Dr. Hirst (as he frankly owns) has found some difficulty in explaining how these various influences transform ' sentience ' into ' percepta '. He tries in turn such notions as ' adjustment ', ' organization ', ' interpretation ', and the like, and thinks none of them wholly satisfactory. But the complex process he has in mind—which, as he says, usually includes the attribution of ' meaning ' to the otherwise meaningless ' sentience '—is what older British psychologists would have called ' apperception ', and have already analysed in considerable detail.[2]

The concluding chapter opens with a ' re-examination of the physiological evidence '. Then, in the light of this, Dr. Hirst states and defends his ' final account of perception '. " Perception," he decides, " is a relation between a person and a public object, in which a mode of active experience, ' perceptual consciousness ', is caused in him by the stimulation of his sense-organs by the object or by emanations from it." Both ' perceiving ' and ' perceptual consciousness ' present two aspects. The inner aspects of each consist in an awareness of an object or scene as external; but their outer aspects are different. The outer aspect of perceiving is a causal chain from object to brain *via* the sense-organs; the outer aspect of perceptual consciousness is " the same brain activity as in the perceiving, or closely similar to it ", but " it does not include any processes outside the body ".

This abbreviated account hardly does justice to the complexity of Dr. Hirst's rather elaborate theory, or to the reasoned arguments by which he seeks to support it. But it will be sufficient to show that, while giving due credit to results obtainable by introspection, he is anxious not to depart too far from the general trend of psychological opinion since the behaviourist movement began. In the first place he seems unreservedly to accept the prevailing view that the essential characteristic of the objects of perception is that they are ' interpersonal ' or ' public '. Secondly, he holds that the critics of the traditional ' bifurcation of nature ' have fully established their case, and is therefore anxious to keep to a hypothesis which will avoid the " faults of dualism ", whether epistemological (as in the ' representative theory ') or psychophysical (as in the ' interactionist theory ').

His attempt to break loose from epistemological dualism has not, in my view, been altogether successful. What he calls ' percipienda ' differ from what other writers call ' sense data ' only by the fact that Dr. Hirst describes them as ' theoretically ' apparent. Thus, after all, his ' final theory ' comes very close to that commonly offered by the more modern exponents of the ' representative theory '. Indeed, as he himself observes, " I have to admit that the *content* of perceptual consciousness does at least *correspond* with the external *object*: I avoid the word ' represent ' as too suggestive of pictorial reproduction " (my italics).

I fancy the difficulties that Dr. Hirst has so candidly acknowledged spring nearly all from the fact that he, in common with most of the contemporary philosophers whom he criticizes, still clings to a psychological account of ' perceptual consciousness ' which is quite remote from the normal experience of everyday life. He and they take it for granted

[1] Readers who are interested in more recent surveys may be referred to D. W. Hammond's *Psychology of Perception*, S. H. Bartley's *Principles of Perception*, or, for a British study of the subject, D. E. Broadbent's *Perception and Communication*.

[2] See more especially G. F. Stout, *Analytic Psychology*, II, pp. 110f. Stout, it may be noted, also uses the term ' sentience ' (defined in his erudite fashion as ' anoetic consciousness ') and adopts it to describe ' sensory presentations ' before they have been modified by the processes of noetic synthesis ' (*ibid.*, I, p. 50).

that the firmest starting-point for an interpretation of perception must be what he calls the ' physiological evidence ', or, as he puts it elsewhere, our knowledge of " the causal processes involved ". Light waves or sound waves emitted by the object eventually reach and stimulate the sense-organ; from the sense-organ electro-chemical impulses travel up the afferent nerves to the appropriate cortical areas. The immediate outcome (it is supposed) must therefore be a kind of ' sentience ' generated by the nerve-tissue, much as a latent picture is generated on the plate at the back of the photographer's camera. This latent sentience is then, as it were, developed and modified in the darkroom of the brain, so as to yield a perceptual ' content ' which ' corresponds to ' or ' symbolizes ' the external object.

Now I maintain that this is far from conveying an accurate impression of what really happens in ordinary everyday perception. In my waking experience I am aware of an ever-changing sensory continuum; and out of this complex and variegated background whatever I attend to at the moment stands forth as a visible, tangible, and more or less accessible object—' out there '. When I look at the bowl of fruit on the side table, I do not see a ' sentient ' reddish-yellow circular patch, ' modified ' into a fruity ' perceptum '; much less do I see a ' sense-datum ' or a sensory ' content ' that ' represents ' or ' corresponds to ' the fruit. What I see is a solid globular orange, just out of arm's reach. And I at once apprehend it intuitively as an independent and relatively permanent thing—an object of which I have, not mediate, but immediate knowledge.

No doubt, if we take as the basis of our reasoning what Dr. Hirst calls ' the physiological evidence ', we shall be tempted to conclude that " what is found at the end of so long and complicated a series of causal events, namely, the perceptual content, is unlikely to be the same as the remote object from which the whole transaction started "[1]. The argument would be perfectly valid if we could identify the ' perceptual content ' with the retinal image, or with an analogous facsimile " generated in the cortex " and then projected into the outer world. But, unfortunately for those who love simple causal explanations and shun anything that savours of action at a distance, what actually happens *is* an immediate and intuitive apprehension of the object as a concrete object seen at a distance from the very start, and not of a mere projected simulacrum. The colour, the shape, even the solidity are directly apprehended as essential characteristics of the thing itself; and we seem forced to recognize a unique kind of cognitive relation linking the percipient to the object perceived regardless of intervening space. And in point of fact the most that we are entitled to deduce from the physiology of sensation is that (paranormal cognition apart) the sensori-neural system acts as an indispensable instrument for perception.

The difference between the philosophical approach and that of the psychologist is clear. The philosopher is supposed to sit in his armchair, close his eyes, and deduce " from the known facts of physics and physiology " the character of the perceptual process. The unprejudiced psychologist opens his eyes, and describes—or should describe —what he actually sees, no matter how paradoxical the description may seem. If we could without more ado accept such an account at its face value, then " the red glow of sunset would be as much a part of nature as the molecules and electric waves by which

[1]This is the dualist's way of stating the conclusion (A. O. Lovejoy, *The Revolt against Dualism*, p. 49). Dr. Hirst contends that the mistake of the ordinary dualist is to " confuse the last stage of the process with the whole ". The dictum quoted in the text is a modernized version of a very ancient philosophical argument: " ' The effect ', it is said, ' must resemble its cause.' Hence, whenever what is transmitted is not a copy of the original emitter (as the scholastics insisted it must be in perception), then what is ultimately received will no longer resemble the emitter." This is an argument that is manifestly contradicted by the success of the telephone, of broadcasting, and of television. There what is ultimately received does resemble " the remote object from which the whole transaction started ". However, in perception—e.g. when I pick up and eat an apple from the bowl of fruit, what I see and feel and taste does not *resemble* the apple; it *is* the apple.

men of science would explain it "[1]; and, I would add, the beauty and the truth of nature would themselves be as objective as the sounds and colours. With this interpretation the contents of the world around us turn out to be far richer and far more complex than the scientist for his own special purposes has commonly assumed. The physical objects of science are mere abstractions from the concrete objects that we perceive—bare skeletons, simplified geometrical or mathematical models, not the luxuriant, richly tinted, redolent, flesh-and-blood realities of our everyday life.

If I am asked, does this mean that ' objects ' still display their colour and their beauty when no real or hypothetical mind happens to be perceiving or thinking of them, and " when there's no one about in the quad ", then I answer that the question is for us quite meaningless, if not self-contradictory. All human knowledge is and must be relative. " Wir können die Dinge nur so denken wie wir sie vorfinden, nie so wie sie jemand vorfindet; wir können die Welt nur von dem Standpunkte aus denken auf dem wir wirklich stehen, nicht von einem Standpunkte aus auf dem wir überhaupt uns gar nicht stehend denken können, oder von gar keinem Standpunkte aus. Ich kann gar nichts absolut denken: im Stillen stelle ich mich noch immer als der Beobachter vor, der die Dinge sich gegenüber sieht."[2] The consequence of this inevitable reservation is to turn the doctrine I have been expounding from a kind of ' natural realism ' into a form of what has sometimes been termed ' objective relativism '.[3] By this I mean that, although what we know is necessarily relative to the knower, it is none the less objective—or at least more objective than subjective. To borrow the language of the statistical psychologist, the observer, as it were, serves as the ' origin ' for a particular ' frame of reference '; and what he perceives varies not only with the variations of the external object; it also varies with the intervening media, and the position or state of the observer's body and mind; but it is *most closely* correlated with variations in the object.

At this point, however, I think what has been called the ' physiological evidence ' ought to be supplemented by what may be termed the ' biological evidence '. Natural selection, in progressively evolving the instrumental apparatus of sense-organs and nervous system, has succeeded in ensuring, not that our knowledge should be perfect or complete, but merely that it shall be adequate for the practical purposes of mundane life under all normal conditions. In exceptional circumstances our ostensible percepts may become more or less confused or even definitely misleading. " Things," as the plain man, like the poet, quickly discovers, " are not always what they seem." In such cases the natural explanation—the one which the unsophisticated thinker usually adopts—is that the relation or process of cognition is itself a somewhat variable or flexible affair. Just as I can aim a

[1] A. N. Whitehead, *The Concept of Nature*, p. 29.

[2] W. Petzoldt, *Das Weltproblem vom positivischen Standpunkte aus*, p. 142. (The pronouncement is really that of the physicist Mach, though the words are those of his philosophical disciple, Petzoldt.) As I have pointed out elsewhere, many if not most contemporary physicists would apply this qualification even to the objects of physical science. " Classical physics," says Heisenberg, " started from the belief—or should we say illusion?—that we could describe the world, or at any rate parts of it, without reference to ourselves." In modern physics we realize that " our descriptions cannot be completely objective ".

[3] The phrase is due to A. E. Murphy; but has usually been treated merely as a theory of perception, whereas I regard it as a statement about all human knowledge: it is also usually taken to imply that sense data are ' universal ', whereas every psychologist, I fancy, must insist that they are ' particulars '. Except for this last reservation what I have said brings my view a little nearer to that which was at one time advocated by Whitehead. But instead of his theory of the object as ' multiply located ' I should prefer to substitute a theory of the object as a pervasive ' field '. Faraday, it may be remembered, held that, according to his theory, " an electric charge is in a sense *everywhere* ". Some atomic theorists have used similar language about their ' elementary particles '. And generally, I believe, wherever traditional science and traditional psychology use the concept of ' substance ', the concept of ' field ' provides a far more useful model.

dart or kick a ball more or less efficiently, so (it would ordinarily be said) I can discern objects more or less efficiently—the efficiency depending largely on the external circumstances and the condition of my own neural apparatus.[1] Dr. Hirst recognizes that in this respect common sense may have hit upon a tenable explanation; but, if I understand him rightly, he is doubtful how far such a theory, in its rather naïve form, can be rendered consistent with the physiological evidence, and therefore feels that it requires the supplementary hypotheses of ' sentience ' and ' percipienda '.

At this stage in the argument, however, I want to emphasize a distinction which hitherto has almost always been overlooked—a distinction that turns on the different ways in which our attention may be directed. Whenever our perceptions become confused or contradictory, our mode of attention alters; its focus is abruptly shifted from the object to the sensory characteristics. The sudden jerk produces a sort of mental dislocation. As a result the sense-datum is apt, as it were, to get detached from the world of things, and to stand out as a self-subsistent *tertium quid*. Instead of being an adjective of the thing we perceived, it becomes a substantive in its own right. When, for example, I try to draw that fruit bowl on a sheet of paper, I find myself seeing the orange as a flat yellow disc, and the rim of the bowl as an oval outline rather than a circular edge. It is, I am convinced, this unnoticed change in the focus of attention that accounts for the sharply conflicting explanations we meet with in all these discussions. The naïve realist keeps to the attitude of the plain man in ordinary everyday life, and his attention is therefore fixed on the object; the philosopher and the introspective psychologist fasten their attention on their sensory experiences, and the inevitable outcome is a dualistic ' sense-datum theory '.

As a scientist, the psychologist's task is not merely to describe experience, but to dissect it. In doing so he tends to analyse the perception of things present rather as he would analyse the remembrance of things past. In the end he is almost bound to adopt something like the representative theory, though I agree with Dr. Hirst that the term ' representative ' is unfortunate. Such a theory can only be adequately expressed by postulating three unique *relations*—relations that are peculiar to cognitive processes as such, namely, the relations between (i) the percipient and the sensum, (ii) the sensum and the percept, and (iii) the percipient and the percept. The percipient *directly* apprehends the sensa; these sensa *imply or signify* a corresponding object; but usually there is a kind of short-circuiting, and the percipient seems *immediately* to perceive the object, and to perceive

[1] As Martin has observed, " ordinary common sense usage naturally embraces such facts as the relevance of the observer and his position, etc., to the apparent characteristics of physical objects " (*Sense-perception and Matter*, 1950, p. 32). Most of the older introspective psychologists adopted some such assumption: the first to reject it seems to have been James Ward. " At first sight," he says, " it looks rather as if the activity might vary, while the object remains the same." But " the theory of presentations " (his word for ' sense data ') requires us to say that it is the ' presentation ' that alters, not the mode of consciousness. In my view the most satisfactory way of answering the familiar epistemological puzzles (those raised by the occurrence of illusions, and the like) is provided by McDougall's version of the doctrine of apperception (cf. his *Physiological Psychology*, pp. 93f.). I tried to indicate how much of this could be formulated in terms of matrix algebra in an earlier article in *Psychometrika*, III, 1938, p. 160, ' The Unit Hierarchy '). " In psychology, as in modern physics, an ' observation ' is to be specified not by a quantity but by a structure, not by the correspondence between a single sensation and a single stimulus, but by the correspondence between patterns or *matrices* . . . We may then regard the real object as specified by the canonical form of the matrices." And " if the entire matrix of stimuli, not the single stimulus, is treated as the unit, the transformations of the pattern " (e.g. the transformations that occur as I change my point of view or the object changes its distance or orientation, and the inverse transformations which my brain unconsciously and automatically carries out in the process of perception to preserve ' constancy ') " are then as simple to examine as other transformations. And the postulate that introspection shall not distort what is observed can be represented by an idempotent operator ".

it as external. This third relation—the relation of perceiving—is, so to speak, the ' relative product ' of the relations of sensing and implying.

By adopting a relational analysis instead of the traditional subject-and-predicate analysis, we avoid the erroneous inference that the sensa are ' ideas in the mind ' (as the older writers maintained), or (as Russell has declared) that they are ' events in one's head '. Nor need we conclude, as Dr. Hirst has done, that, because ' perceiving ' (in the sense of the physical, physiological, and psychological conditions underlying the final cognitive relation) " is a highly complex process ", it therefore follows that " perceiving " (in the narrower sense of the resultant cognitive relation) must be " far removed from simple awareness ". Puzzling as it may seem to the non-psychological thinker, the fact remains that in normal circumstances perception does unquestionably possess something of the sheer intuitive character of ' simple awareness '.[1]

This view, I believe, could be fully substantiated by a survey of the relevant results from experimental work on perception and attention. But even were we, with Dr. Hirst, to reject all forms of the sense-datum theory, we still could not completely escape a dualistic conclusion. Suppose, for the sake of argument, we agreed that all sensory qualities are intrinsic qualities of the physical object or its surfaces and not the attributes of certain non-physical entities called ' sense-data ', we should still have to recognize the occurrence of mental images in memory, of hallucinations in delirious and psychotic states, and of all the stuff that dreams are made on; and these are unquestionably non-physical phenomena. What is more, the processes of sensing and cognizing, not to mention feeling and desiring, are from their nature non-physical. To maintain that these and other mental activities are merely the ' inner ' aspects of material processes in the brain, and that the two types of process are therefore in fact ' identical ' is surely no more than a verbal subterfuge. The very use of such antithetical terms as ' outer ' and ' inner ' is a tacit admission of their disparity.

There is one further criticism I should like to make. Throughout his account Dr. Hirst seems to suppose that, whereas the ' sense-data ' assumed by his opponents must of necessity be ' private ', the ' object ' as such is defined by the fact that it is ' public ', i.e. (as he puts it) " shared by other members of the community ". Although many psychologists would agree in making publicity the main ground for believing in the existence of ' external objects ', such an interpretation is to my mind quite mistaken. If, like an indigenous Robinson Crusoe, a new-born infant could survive in perfect solitude on a desert island, the child would still see the things around it—the rocks and the palms, the parrots and the goats—as objects external to himself. The real ground for distinguishing physical objects from our sensations or perceptions is that they are felt to enjoy a continuous and relatively permanent existence of their own, whereas our perceptions are of necessity intermittent, fragmentary, and subject to perpetual interruption. The primary function of the physical object is to fill in the temporal gaps; their second function is to furnish an intelligible and more or less consistent causal basis so that prediction becomes possible. They

[1]Cf. Burt, ' The Concept of Mind ', *J. Psych. Res.*, I, ii, 1960, pp. 1–11. Although I have described the relation of perception as a ' relative product ', I should still contend that in itself it is unanalysable: it is not, like the relation between uncle and nephew, *reducible* to two logically prior relations. Nor can I regard it (with some writers) as a special kind of three-term relation: e.g., " the *orange* appears *yellow* to *me* ". This type of statement occurs as a natural description only in somewhat exceptional circumstances (e.g. when there is a debate about the apparent quality), and cannot be erected into a theory covering all forms of perception. It should be noted that the three statements (i) ' This object which I now see appears yellow to me ', (ii) ' What I now see appears to be a yellow object ' and (iii) ' The object which I now see is a yellow appearance ' (' the basis of the sense-datum theory) are repeatedly treated as equivalent; actually they are very different judgements.

are, in the statistician's jargon, ' invariants '. Their so-called ' publicity ' is an incidental feature: indeed, the plain man attributes just the same publicity to sensory qualities as he does to physical objects.

Most of the criticisms that I have ventured to make turn on relatively minor issues; and often, I suspect, the difference between us resolves largely into a matter of wording. The weakest point in Dr. Hirst's argument is, in my view, his attempt to remove the paradoxes of perception by invoking an Identity Hypothesis. But even here he is searching in the right direction, though what he finds is hardly the proper key to the puzzle. And he has certainly brought out a most important point that philosophical writers have been prone to overlook—namely, that the first requisite for a satisfactory solution must be a sound psychological theory based on a comprehensive survey of the relevant psychological facts. All psychological students, therefore, should be encouraged to read this illuminating survey, and consider for themselves the ingenious hypotheses that Dr. Hirst has here put forward.

CYRIL BURT

[1]I use, as Dr. Hirst does, the phrase ' physical object' as the most intelligible in this context. Strictly speaking, however, that should be reserved for 'the object as conceived by physical science '; and I should prefer the term ' perceptual object'. But that would mean needless complications in my account.

BOOK REVIEWS

Individual Choice Behaviour: A Theoretical Analysis. By R. DUNCAN LUCE. New York: John Wiley & Sons; London: Chapman & Hall, 1960. Pp. xii+153. 48*s*. net.

Acts of choice, implicit if not explicit, enter into a wide variety of topics studied by the psychologist—discrimination, learning, information theory, decision theory, utility theory, and almost every form of motivation. Professor Luce's aim has been to develop a general mathematical theory applicable to all these different fields. Throughout his discussion he assumes that the situations with which the psychologist is concerned are of the following type: there is a relatively homogeneous ' universe ' of alternative possibilities—it may be a specific range of stimuli, as in experiments on sensory discrimination or subjective preference, or a specific range of responses, as in studies of learning and of motivation. This ' universe ' (U) is such that, out of certain finite subsets which it contains, a subject can select that particular alternative (or group of alternatives) which he considers to be the most appropriate as judged by some definable criterion (e.g. relative loudness in the case of auditory stimuli, likelihood of relative satisfaction in choosing alternative modes of behaviour). After reviewing the different types of problem arising in such situations, Professor Luce arrives at the conclusion that ' choice behaviour ' may best be treated, not after the manner of previous workers as an ' algebraic phenomenon ', but rather as a special type of ' probabilistic phenomenon '.

His book is divided into four main chapters. The first sets forth the basic theory. Since the process involved is somewhat complex, his method assumes, for reasons that become clearer in the course of the subsequent discussion, that the final selection can be analysed as though it had been reached by stages. To take an example from a recent British experiment on artistic preferences, Miss Williams showed a set of nine pictures individually to a class of children, and required them to work by successive steps. Each child was asked first to select the three he liked best, and the three he liked least, leaving three about which he had no strong feelings one way or another. Then with each subset of three he was invited to make a similar threefold choice—i.e., to pick the one liked most and the one liked least. The ultimate result was to get the whole series of nine ranked in order of preference.

In setting out his formal proofs Professor Luce begins by enunciating what he calls the fundamental ' choice axiom '. This is expressed in the usual symbolic notation, and lays down the basic conditions of independence and transitivity. Translated into verbal terms, the first part of the axiom may be roughly interpreted as follows. Suppose that R is a small subgroup of possibilities (in a test of aesthetic preference it might be just a single picture), and that R is contained in a wider subgroup S (e.g., a batch consisting of three pictures), and that S in turn is contained in the total sample T (in our example the whole set of nine pictures): then the probability that a subject will select R out of T, namely $P_t(R)$, will be equal to the product of the probability that he first selects S out of T, i.e. $P_t(S)$, multiplied by the probability that he then selects R out of S; or in symbols.

$$P_t(R) = P_s(R)P_t(S).$$

If the various probabilities are estimated on the basis of proportionate frequencies obtained from a reasonably large number of subjects, then it is obvious that in certain somewhat trivial cases this part of the axiom would be virtually a tautology. In cases in which the selections are strictly random, the equation states the familiar theorem of ' compound probability ' for two independent events. It is, however, by no means difficult to imagine certain realistic situations in which the formula might break down rather badly.

Nevertheless, intuitively at least, it would certainly seem plausible as a working hypothesis for clearly defined sets in which the number of alternatives is comparatively small. Moreover, such a restriction presents no serious drawback, since (as it turns out) Professor Luce's own deductions merely require that the axiom shall be true for sets containing at most three alternatives.

The second part of the axiom simply states that, if y is invariably chosen in preference to x, then x may be deleted from T whenever we are considering a choice from T. To take Professor Luce's example, " if I always choose liver in preference to beef, then for me the choice between the three items—beef, liver, and chicken—can be reduced to a simple choice between liver and chicken ". It is surprising to find what a number of interesting consequences, some of them quite unexpected, may be deduced from this twofold ' axiom '.

A crucial problem in the study of choice behaviour is the construction of a numerical scale which will represent the behaviour of the individuals concerned. In the past various procedures have been suggested, and provisionally adopted, by economists, by psychologists, and by psychometricians. All are open to criticism. Professor Luce therefore proceeds to demonstrate that, for pairwise discrimination, his axiom implies the existence of a *ratio scale*, i.e., a linear scale that is unique except for its units. His first step is to formulate an existence theorem for a ' local region ', i.e. for subsets of T: then, by using overlapping subsets, he shows how the ' v-scale ' (as he calls it) can be extended over the whole range of the ' universe ' of stimuli or responses with which the investigator may be concerned.

The remaining chapters—in many ways the most illuminating—deal in turn with the application of his ' basic theory ' to the commoner problems of psychophysics, of utility theory, and of learning theory. The properties of the more important models are examined; and at various points practical suggestions are put forward in regard to the best methods of estimating the underlying probabilities and the requisite parameters (e.g. by applying the method of maximum likelihood). From time to time Professor Luce discusses ' previous work '; but here his references are for the most part confined to American publications. For example, the " rather complex, normally distributed, but unobservable subworld " postulated by Thurstone as a basis for his scaling methods is described and criticized in some detail; but the improved scaling procedures based on R. A. Fisher's principles are never mentioned. There are instructive discussions of ' rank ordering ' and the method of ' paired comparison ', but no reference to the work of Kendall and his fellow-workers. However, it was not part of Professor Luce's intention to offer a comprehensive survey of alternative modes of approach, but merely to illustrate the nature and merits of his own; and this he has done with great ingenuity and skill.

In this brief outline of his general procedure I have tried to interpret it from the standpoint of British work, and to see it in the context of the problems that have interested the British investigator. I hope that in so doing I have not too seriously distorted the strict and rigorous exposition to which he himself has adhered. It is undoubtedly much to be desired that psychologists in this country will turn their attention to the aspects of the question he has raised and to the mode of solution he has advocated. He has himself, in his final appendix, summarized the more urgent of the many unsolved issues that still remain. Of these perhaps the most obvious is the need to test the validity of his basic axiom by *ad hoc* investigations, and to determine the special conditions under which it holds good. For stepwise scaling I would suggest the binary method of ' expressing as bipolar factors ' (Mis *Journal, IV*, p. 196). Still more important, though still more difficult as Dr. Luce points out, is the task of developing an adequate procedure for " describing the way organisms subdivide their environments into chunks and establishing the laws that govern the relations of one sub-division to another ". Research along these lines can hardly fail to be fruitful.

CYRIL BURT

Modern Factor Analysis. By HARRY H. HARMAN. Chicago: The University of Chicago Press, 1960. Pp. xvi + 469. $10·00.

Twenty years have passed since Professor Holzinger and Dr. Harman first published their ' Synthesis of Factorial Methods ' under the brief title of *Factor Analysis*; and during that interval the whole subject has made considerable strides. A new edition of the work had already been planned, when, to the deep regret of all his colleagues both in America and over here, Dr. Holzinger died. The new volume is something more than a mere revision. Among the novel features perhaps the most significant are the detailed accounts of methods of high-speed calculation which have resulted from the introduction of electronic computers. Analytical methods of rotation and statistical procedures for testing hypotheses are described in some detail. An entirely new ' part ', consisting of 40 pages of ' problems and exercises ' (with answers) arranged according to chapters, has now been added; and the bibliography has been enlarged to over four hundred entries, including thirty papers published in this journal.

To make room for these additions, much of the earlier volume has been omitted or abridged. And at the same time there seems to have been a change of attitude in regard to factorial techniques. With British investigators one of the commonest factorial schemes has always been that which includes first a ' general factor ' and then a set of ' group factors ', often overlapping, and not infrequently subdivided into still smaller ' group factors '. This type of pattern is now only mentioned in passing; and the account of Holzinger's own ' bifactor method ', which gave a simplified scheme of this kind, has been greatly condensed. On the other hand, the discussion of the methods and concepts associated with the ' Thurstone school ' (notably ' simple structure, which formerly was dismissed in a short footnote) has been expanded to over a hundred pages. Nevertheless, the book as a whole is admirably planned, and the exposition is in the main clear and impartial. There can be little question that the volume will at once become one of the standard textbooks on factor analysis. Indeed, in the reviewer's opinion, it is by far the best at present available.

CHARLOTTE BANKS

Information and Error. By SOLOMON DIAMOND. New York: Basic Books, Inc., 1959. Pp. xii + 307. $5.00.

" Whoever wishes to explore the mysteries of modern psychology," says Dr. Diamond in his preface, " must carry with him an equipment of statistical techniques that were esoteric twenty years ago; and this situation calls for a new approach in the teaching of statistics." He believes that the most urgent need is a change to an ' analytic viewpoint ' as distinct from a ' descriptive viewpoint '; and it is the latter, so he maintains, that has dominated statistical teaching in psychology hitherto. The older textbooks were restricted to a study of probable or standard errors and correlational techniques; the newer textbooks tend to pass these by, and deal by preference with those procedures that are based on socalled analysis of variance. Accordingly, he has decided to include within the covers of a single textbook both the earlier Pearson-Thorndike approach (slightly modernized and supplemented with detailed discussions of Thurstonian factor analysis) and the newer methods of the Fisher-Snedecor school (supplemented with a final chapter on ' non-normal data ') and to rewrite both from his ' analytic ' standpoint. At the same time an attempt is made to enliven the algebraic exposition with touches of American humour. The more technical section headings—' null hypothesis ', ' chi square ', ' regression formulae ', and the like—alternate with quizzical titles like ' the professor's quandary ', ' the loving sons ', and ' the tale of the scatterbrained boy '. The British student does not, as a rule, greatly relish facetious interpolations in his textbook; but he appreciates a little light relief in his lectures. And psychologists who have to discourse the somewhat arid problems of statistical techniques may find in Dr. Diamond's book much that is suggestive as regards both subject matter and treatment.

CHARLOTTE BANKS

Behaviour. By D. E. Broadbent. London: Eyre & Spottiswoode, 1961. Pp. 215. 21*s*. net.

Even the sternest critic of behaviourism as a basic psychological theory must pay his tribute to the wealth of information which behaviourism as a method has furnished about some of the most elusive problems of psychology. Indeed, owing to the constant controversies between the leading members of the school the sum total of their joint contributions has, if anything, been underrated. It is the purpose of Mr. Broadbent's book " to summarize the achievements of the behaviourist approach up to the present day ". As a general review it is comprehensive, critical, and thoroughly impartial in its recognition of the merits of the rival theorists. It is full of suggestive analogies and illuminating asides, and spiced with unexpected touches of humour. The style is easy and non-technical, so that not only the student of psychology, but also the general reader can follow the arguments. The criticisms are inspired by a robust common sense, and well worth the attention even of the specialist who is already acquainted with the relevant facts.

Behaviourism was in many ways a youth movement ; Watson and most of his followers were then in their early thirties. It started as a protest rather than as a fresh discovery or a novel theory. Accordingly, Mr. Broadbent begins his book with a summary of the chief defects of ' introspectionism ' as seen through the eyes of Watson when he first launched his attack half a century ago. The introspective method, it was alleged, had produced nothing but conflicting results; it provided no explanation for its own findings; and it offered no satisfactory procedure for studying or interpreting the behaviour of animals. Such complaints may have been warranted in the case of American or German psychology—as exemplified by the inconclusive controversies over the James' description of emotions and the Würzburg doctrine of imageless thought (to quote Mr. Broadbent's own illustrations). But the criticisms were scarcely justified in the case of British psychology.

Long before the date of Watson's celebrated manifesto, McDougall had rejected the traditional definition of psychology as ' the study of human experience ', and had described its true aim as ' the explanation of the behaviour of men and animals '. He too declared in no uncertain terms that " we cannot hope to arrive at an adequate understanding of behaviour by the methods of introspective psychology alone ".[1] Years before Watson published his first paper,[2] students like myself and my contemporaries had learned from McDougall at Oxford or Myers at Cambridge the need for " objective methods of experiment ", which Mr. Broadbent cites as Watson's ' central belief '. We had been warned of Lloyd Morgan's canon—" not to interpret an animal's behaviour in terms of higher quasi-human processes, when it can plausibly be accounted for by lower quasi-mechanical processes of a simpler type ".[3] We had heard about the irreconcilable conclusions of different introspectionists; but we were told such conflicts were only to be expected in view of the wide individual differences which Galton himself discovered in almost every field, particularly in his studies of imagery and emotion. Stout, as well as McDougall, would have readily agreed that self-observation alone could never fully explain what self-observation might reveal. It was on this very ground that they put forward the theory of ' psychoneural dispositions ', based largely on Sherrington's neurological experimental studies of elementary reflexes. From Sherrington himself we heard, long before Watson mentioned them, about the researches and speculations of Pavlov.[4] And in McDougall's theory of the learning process we encountered (couched in

[1]W. M. McDougall, *Physiological Psychology*, 1905, pp. 1–5. It is curious to note that the very wording is almost identical with Mr. Broadbent's.

[2]A monograph (1907) dealing with the ' sensations ' (sic!) of the rat in solving the problems of the maze.

[3]Ll. Morgan, *Animal Behaviour*, 1900, p. 270.

[4]The most important of Pavlov's early papers (*Ergebn. d. Physiol.*, III, i, 1904, pp. 177f.) is cited in Sherrington's *Integrative Action of the Nervous System*, 1906. We there learn too of the

slightly different terms) nearly all the competing partial theories put forward by the different exponents of post-Watsonian behaviourism.

With most of this, I imagine, Mr. Broadbent would fully agree. His purpose has been, not to write a history of psychology, but to record and evaluate the fruits of the new method of attack. What then were the novel or distinctive points in Watson's own teaching? Three main principles, we are told, stand out: the rejection of all introspection, a belief in the superiority of environment over heredity, and a simple faith in conditioning as the supreme explanatory process; and Mr. Broadbent proceeds to show how each of these principles has been progressively modified in varying degrees by the later members of the school.

To his English contemporaries Watson's earlier pronouncements seemed at first to be little more than a revival of the Spencerian type of associationism which had been so drastically criticized by Ward and Stout. It had been the claim of the British associationist school to have discovered the ' laws of association '; these, it was maintained, governed the processes of learning and therefore all forms of adaptive behaviour; and it was the cherished ambition of nineteenth century psychologists to reduce these laws to a minimum number of basic principles. Those who accepted the threefold division of mental process commonly recognized three: (i) the cognitive law of temporal contiguity (usually supplemented by a law of ' exercise ' or ' frequency '), (ii) the conative principle of motivation or drive, and (iii) an affective law generally known as the pleasure-pain principle (usually expressed in terms of ' satisfaction ' or ' dissatisfaction ' which were supposed to reinforce or eliminate the ' associations ' they accompanied). Watson discarded the last two on the ground that they attributed a causal agency to consciousness, and retained merely what Broadbent calls " the conception of stimulus-response association by contiguity". The term ' conditioning ' was used to designate the hypothetical process of ' switching ', whereby a new reflex stimulus or a new reflex response was (in Watson's phrase) ' substituted ' for the first.

However much we may criticize the details of Watson's teaching, we must give him full credit for the widespread enthusiasm his exhortations inspired. But during the next ten or fifteen years a vast accumulation of experiments, carried out by an army of independent workers, gradually made it clear that his extremely simple hypothesis left a wide variety of results wholly unexplained. And it became obvious, as Broadbent demonstrates by convincing instances, that " Watson's neglect of motives and rewards had gone too far ". At length, in the early thirties, another theory of learning was put forward, with the 'law of effect ' as its central principle. The ' effect ', however, of reward or of punishment was now treated as purely mechanical.

This change in standpoint is generally associated with the name of Clark Hull, a comparatively late recruit to the behaviourist fraternity. His aim, as he tells us, was " to reach a quantitative determination of the laws of behaviour and to arrange them in a deductive system ". The underlying theory, formulated with all the logical rigour of an ' axiomatic procedure ' (rather like Spinoza applying Euclid's methodology to problems of human conduct) is in its basic nature very similar to Lloyd Morgan's account of habit-formation through trial and error. Broadbent here draws a most instructive parallel. The doctrine of natural selection maintains that new species are evolved as a result of chance variations in individual structure, among which only the best adapted survive; the law of effect maintains that new habits are acquired as a result of chance variations in transitory reactions, out of which none but the most appropriate are preserved. And he drops a suggestive hint for the statistical psychologist. " Curiously enough," he says, " nobody seems as yet to

importance of conative *driving force* and of affective *reinforcements*, and how reflexes may be modified by the coordination of one reflex arc with another. However, we talked of ' associated reflexes ', not of ' conditioned reflexes ', so there was as yet no popular slogan to publicize the new and fruitful procedures devised by the Russian physiologists.

have applied to animal behaviour the mathematical arguments used in dealing with the evolution of species."

However, still further experiments quickly revealed three types of case in which the results observed seemed quite inconsistent with the new doctrine, viz., " first, failure to learn even when a reward was given; secondly, successful learning when no reward was given; and thirdly, the learning of a mode of action different from the action that had been rewarded ". There is one characteristic type of behaviour observable in the rat—the animal most frequently studied by psychologists in their work on learning: to most of us it is familiar from the habits of our domestic pets, namely, their use of exploration as a means of spontaneous learning. Put a hungry rat into a strange maze. Its first reaction is, not to run straight for the food that it can smell or see, but to inspect and investigate its new quarters. It thus acquires, as a few simple tests will show, a systematic knowledge of its surroundings. There is here no question of punishment or reward, unless with McDougall we regard a satisfied curiosity as its own reward. The non-psychological reader will think there must have been some kind of blind-spot in the eyes of the earlier observers, if (as Broadbent assures us) so conspicuous a mode of behaviour was ignored for thirty or forty years. The advantages of this instinctive propensity can readily be understood in terms of a new concept put forward by Tolman. His own name for it was ' sign-Gestalt-expectation ', which Broadbent neatly translates as ' the map-in-the-head theory '. Tolman, so Gardner Murphy remarks, " agreed with McDougall " (and it might be added, with biological investigators like Tinbergen and Lorenz) " in emphasizing the need for a *purposive* approach: the organism perceives objects as means towards goals ". The most distinctive feature in Tolman's new approach consists in the fact that he asks us to turn our attention, not so much to the motor or response-aspect of the total process, but rather to the cognitive or perceptual aspect.[1] In its generalized form it is plainly capable of accounting for many phenomena which defied interpretations in terms of the older and simpler doctrines.

One of the chief reasons given by Watson for rejecting the ' subjective ' and ' anthropomorphic ' terminology of the introspectionist school was (in Broadbent's phrase) that " it made comparison with animals difficult ". At the same time the behaviourists' virtual rejection of hereditary factors has led to " a neglect of innate behaviour " and a concentration on " the mechanisms of learning " as studied in the laboratory. If, however, one extends one's observations from behaviour in the laboratory to more natural conditions, and from the humble rat to the higher carnivores and primates, or even to the domesticated cat and dog, then, I am tempted to say, the older mode of approach really makes comparison easier. There is a valid as well as an illegitimate type of anthropomorphism; and the psychological concepts we use should be applicable to our fellow-mammals as well as to our fellow-men. This was Darwin's attitude; and he for one felt no scruple over speaking about " emotions in man *and animals* ". " Higher vertebrates," says Julian Huxley, " have behaviours similar to ours in many essential ways: why deny them psychological experiences similar to ours? To give the fullest possible interpretation to their behaviour we must have recourse to a language that employs a subjective as well as an objective terminology."[2] Mr. Broadbent, I imagine, would largely agree. He too maintains that " quite complex actions may be necessary to the animal just as primary or physical ' needs ' are—a fact which a psychologist studying rats in mazes might never have discovered ".

[1]Tolman himself relates that he " borrowed the notion that purpose and cognition are essential ingredients from Perry "; and his own efforts were largely concerned with " translating familiar pre-behaviour concepts, such as ' conscious ', ' emotion ', ' perception ', etc., into new, non-physiological behavioural terms . . . free from introspective definitions " (*History of Psychology in Autobiography*, IV, 1952, pp. 332f.).

[2]Introduction to Joy Adamson's *Living Free* (1961), pp. x-xi.

Broadbent goes on to emphasize a further weakness in the work of these earlier theorists: they seem to be not only out of touch with the relevant work carried out by contemporary biologists (notably those who were studying instinctive behaviour); they also showed " a curious lack of any connection with physiology". And equally, he says, the textbooks of physiology evince a curious ignorance of recent psychological work: " they still give Pavlov's interpretation as though it had remained unquestioned ". A strenuous attempt to overcome this defect was made by Lashley: every student knows how his experiments on rats and other animals threw the gravest doubts on the old concept of cerebral localisation. " The first impact of this finding," says Broadbent, " was to encourage the view that regarded only patterns as crucial." In consonance with the earlier speculations of Köhler and his fellow Gestaltists, " the incoming messages from the senses were now thought of as setting up fields of electrical potential on the cortex ".

But meanwhile evidence was rapidly piling up to show that what Adrian had called " the all-or-none principle in nerve-conduction " holds good, not only of afferent and efferent neurones (where it can readily be demonstrated), but also of cortical neurones. This conclusion seemed hardly compatible with the electrical field-theory, at any rate in its simple form; and later experiments by Lashley and others " cancelled out the boost which his earlier experiments had given the theory ". Hebb therefore suggested an alternative picture for the way behaviour is ' organized '. To me, I confess, his descriptions of ' cell assemblies ', ' neural after-discharge ', and ' phase sequences ', instructive as they are, appear little more than a schematized version of the old theory of the ' integrative action of the nervous system ' particularly as it was developed by McDougall in his account of how ' psychoneural dispositions ' are formed at the higher levels—though, of course, Hebb's formulation is expressed in a novel and more up-to-date terminology. But, as Skinner and other critics were quick to point out, it still remains almost entirely hypothetical.

At the present time, we are told, enthusiasm for these various extensions of the original behaviouristic doctrine is on the wane. Today the methods most frequently used, and the interpretative principles most widely adopted, are those of Skinner, the youngest of the behaviourists we have so far mentioned. In many research laboratories the ' Skinner box ' has almost entirely ousted the manifold versions of the rat-maze. Skinner, like Thorndike of old, and like Mowrer, Maier and Schneirla, and several other contemporary writers, overcomes one of the long disputes which divided the earlier behaviourists by recognizing that both the rival theories contain an element of truth. There are, he maintains, *two* basically different types of learning process—by mere ' contiguity ' and by ' reinforcement ' (or ' effect ') respectively, though he gives each of the traditional principles a slightly novel twist. In Skinner's terminology the former is called ' respondent ' or ' classical conditioning ', and the latter ' instrumental ' or ' operant conditioning '. One of his special devices is to programme the method of learning by subdividing it into elementary steps, each with its own reward, so that the creature learns the final form of behaviour without any needless error—a principle more recently embodied in his ' teaching machines ' for human pupils.

Above all, Skinner has endeavoured to adhere strictly to the principles of ' operationism ' (in Bridgman's sense) and to eliminate the various ' un-point-at-able ' structures and processes that had crept back into the later explanatory theories. Instead, he seeks to discover ' invariable deterministic ' laws, free from all contamination with ' chance factors '. However, as Mr. Broadbent justly points out, a methodology of this strict and narrow type is almost bound to leave out much of what we really know, or can plausibly infer, about behaviour and its underlying mechanisms. He criticizes in some detail Skinner's " hostility to statistics "; after all, " the *variability* of behaviour is one of its most crucial features ".

Most of the foregoing developments, which Mr. Broadbent has reviewed at length, and I have epitomized so far as possible in his own words, will already be familiar to the reader. But it is precisely his way of wording or rewording these familiar details which is

to my mind so instructive. Often by a novel turn of phrase he succeeds in bringing out a fresh aspect in what was otherwise well known; and at every point his explicit criticisms are based on an unbiased judgement and sound practical sense. His applications of the results to human behaviour are always enlightening.

His last chapter looks back over the field, and sets out his final conclusions in regard to the behaviourists' achievements and their chief weaknesses and flagrant omissions. " Facts in profusion have been discovered, some surprising, but most of them in accordance with traditional wisdom "; but the outstanding merit of the movement has been to provide " objective means of testing intuitive knowledge ". Its most obvious defect is that " almost all the research has been concentrated predominantly on the rat." Most of it has been laboratory research. Unlike the zoologist, the behaviourist rarely starts by " observing an animal in its natural surroundings ". " Another besetting weakness of behaviour theory has been . . . its tendency to premature formalization". The majority of theorists, notably Clark Hull, have put too much faith into the ' hypothetico-deductive procedure ' (or, as Mr. Broadbent dubs it, ' the guess-and-check method ') which, as he repeatedly reminds us, commonly resulted in a neglect of other possibilities. " Alternative theories should be set up, instead of putting forward just one guess alone." A promising way for overcoming these limitations would be " to use more sophisticated mathematics: with mathematical reasoning it would be impossible to make false inferences or to rely on concealed assumptions to such an extent as theories of behaviour have done in the past ". The statistical investigations of Estes, and of Bush and Mosteller, are cited as illustrating what may be attempted along these lines. But the mathematical analysis should be " based on experimental work of comparable rigour and scope ".

" Perhaps the worst weakness of behaviour theory is its method of dealing with events inside the brain, revealing themselves only indirectly in action." The recent theoretical work on electronic machanisms which carry out guidance and control, or undertake the analysis and transmission of information, might, he believes, provide a useful descriptive terminology—a view developed with much skill in his earlier volume. I am tempted to make one further suggestion. Time after time the development of behaviour theory, as we have seen, has proceeded by reintroducing, under new and non-committal names, concepts in regular use among psychologists of the introspective, pre-behaviouristic school. Many of those who have studied the problems of learning among human beings—particularly among children—would be inclined to urge that the ban on introspective checks and on theories frankly expressed in introspective language should now be lifted. It is quite as easy to check the results of self-observation as to check the results of observing rats. Mr. Broadbent himself contends that " it is the quality of his experience which makes Man so valuable "; and (if I understand him rightly) he would not deny that much of this can be " communicated in the language of science ". Indeed, he turns the tables on those critics of behaviouristic methods who hold that such doctrines tend to " undermine morality and responsibility " by declaring that " if we were to reject the scientific approach because it is not private, we should also be rejecting all attempts to share the experience of others, whether artistic, religious, or of some other kind ": the very attempt to communicate such experience " must rely ultimately on the description of situations and actions ". After all, he asks, " is it not egocentric to have no curiosity about the richness and variety of other people ? "

CYRIL BURT

The Language of Psychology. By GEORGE MANDLER and WILLIAM KESSEN. New York: John Wiley & Sons. London: Chapman & Hall, 1959. Pp. xviii + 301. 54*s*. net.

" This book," as the authors explain, " is intended to be a compound of the analytic and behavioural view of science," and with this as their twofold guiding principle they have

sought to " provide the student with an introduction to the language of science in psychology and the social sciences." The analytic standpoint is that of American logicians, like Quine and Hempel, rather than that of the earlier British champions of linguistic analysis; and the behaviourist standpoint is that of Hull and Skinner rather than that of Watson and his immediate followers.

The book is divided into two main parts. The first deals with ' the vocabulary of psychology '. The idea of a " psychological vocabulary rooted partly in immediate phenomenal experience " is rejected on the familiar ground that experience is private and not public. The study of perception is not absolutely banned, since after all science is based on perception; but we are told that it must " be dealt with in terms of verbal reports ". After a still more detailed consideration the authors also decide that neither a " commonsense thing-language " nor a " purely physicalistic language " would be sufficient by themselves. In the end they conclude that a " basic criterion " is needed to decide whether or not a word or phrase is admissible as a " scientific term "; and they maintain that the only satisfactory test is " response invariance "—a description which covers precision, reliability, and consistency as regards both ' intra-subjective ' and ' inter-subjective ' usage. The term ' response ', be it noted, includes internal or ' organismic ' responses as well as overt or external responses.

The second part is entitled ' theory in psychology ', and deals with the general nature of scientific theories, and with the characteristics, sources, and uses of explanatory systems. This leads to a full discussion of both induction and deduction as methods of scientific proof. ' Intervening variables ' and ' disposition terms ' are permitted; but they must be rigorously defined by means of ' reduction sentences '. However, anything like a thoroughgoing reductionism in psychology, such as was advocated by Carnap, is deemed impracticable. The volume ends with an interesting chapter on ' special problems in the language of psychology ', including a short but excellent account of scientific ' models and analogies '.

It will be seen that the criterion which the authors propose is capable of a fairly generous interpretation. Nevertheless, to insist on a vocabulary that is primarily behaviouristic—to exclude all " entity-characteristics except those for which there are theoretical or proto-theoretical physical justifications "—seems to be needlessly restrictive. It leaves out too much. As several of his critics have argued, " to invoke (as Watson did) ' internal ', ' implicit ', or ' organismic ' responses which by hypothesis are unobservable is far more precarious than frankly admitting introspective descriptions, which, in principle at any rate, can be readily checked by the observations of others; and to treat these introspective descriptions as no mere ' verbal responses ' is to divert attention to the wrong problems—to what the subject says instead of what he experiences ". After all, provided our subjects are adequately trained, their most illuminating contributions are the processes they observe, not the manner in which they communicate their observations.

However, in spite of these minor defects, the book as a whole undoubtedly succeeds in bringing together in a most helpful way the chief points made by the modern school of linguistic analysis, and in showing how they bear on the task of the psychologist. It is instructive to discover that, as a result of their own critical and impartial survey, and almost (it would seem) in spite of themselves, the requirements which the authors finally lay down are decidedly more elastic than those of the behaviourists and analysts whose somewhat dogmatic pronouncements provided the main stimulus for their work. At times their own language is liable to surprise the reader by the way their metaphorical vocabulary gets mixed: (e.g., " this curtain-raiser to the new psychology, against the background of Helmholtz's and Fechner's work on vision and psychophysics, has remained a major millstone of modern psychology "). Nevertheless, in spite of the abstract nature of the subject, the discussion as a whole is clear, readable, and fully illustrated with apt examples.

M. Howard

An Introduction to the Principles of Psychology. By B. R. Bugelski. New York: Rinehart & Co., 1960. Pp. xviii + 571. $6.00.

" This text," the preface tells us, " has been prepared for the qualified and serious student: it is doubtful that a smattering of ignorance should be presented to the non-major." From this the reader will gather that Dr. Bugelski's rather weighty volume is typical of the many introductory textbooks intended for the American classroom. Psychology is defined as " the scientific study of behaviour, internal and external, of organisms ". Because of their ' subjective connotations ', the use of terms like ' experience ' and ' mind ' is deprecated. The general standpoint is that of J. B. Watson as developed by C. L. Hull and more especially by B. F. Skinner. In the opening chapter Skinner is quoted as claiming that " by the proper use of psychology all students can earn A grades if properly taught: delinquency, disorderliness, and undesired modes of behaviour can be eliminated "; and the student is urged to think of psychology as a profession like medicine, and not merely as a science.

In the body of the work, however, the behaviourist views are put forward in a more tentative and flexible fashion than is customary with writers belonging to the same school of thought. When it comes to summing up arguments on such controversial issues as ' nature or nurture ', ' the evidence for extrasensory perception ', and the like, the conclusions are frequently phrased in the form of rhetorical questions rather than dogmatic affirmations, or expressed in terms of ' might be ' or ' may be '. Indeed, without the help of the ' instructor ', the attentive reader will often find himself in the dark as to what exactly is the final upshot. There are several curious errors in the spelling of names; the familiar doctrine of the four temperamental types, for instance, is ascribed more than once to ' Hypocrates '.

The student is introduced at an early stage to the elements of statistical psychology, and a chapter is devoted to ' psychometrics '. But here and elsewhere the views of older investigators—Galton, Binet, or Spearman—are not always quite correctly stated. Spearman, for example, is said to have maintained in his 1904 article that " intelligence must be thought of as consisting of a general component, G, and some unknown number of special aptitudes, S ": (Spearman, of course, denied the existence of special *aptitudes*, and identified ' intelligence ' solely with G). Much is made of the fact that intelligence as tested varies with the individual's scholastic and social environment; but, when we learn that the tests that the author has in mind " rely heavily on vocabulary items " and " call for items of information ", such a result is hardly surprising. However, the final conclusion is that " intelligence *may be* an intervening variable ", and " it *might be* that inheritance sets some kinds of limits " (reviewer's italics).

One attractive feature is the accompanying booklet which contains a series of ' new type questions ' on the topics of the several chapters, so that the student can test his knowledge and check his answers. A ' work book ' is promised which will " incorporate as much as possible of the ' teaching machine ' design ". J. M. Davis

Behaviour Genetics. By John L. Fuller and W. Robert Thompson. New York: John Wiley & Sons. Pp. x + 396. 72*s*.

It is remarkable that no psychologist has hitherto attempted a comprehensive synopsis of recent work on the facts and theories of mental inheritance. Biologists, from the time of Darwin and Galton onwards, have consistently maintained that heredity has much the same influence on the physical bases of mind as it has on other physical characteristics. Psychologists, on the other hand, have drifted away from the views their predecessors held, and today for the most part seem to have decided that genetic conditions can have

little or nothing to do with determining the mental differences between one individual and another. The reason for this ' anti-heredity bias ', so Dr. Fuller and Dr. Thompson believe, " springs largely from the social and political implications of the subject ". But it has surely been also encouraged by the increasing influence of behaviouristic doctrines during the last two or three decades.

Even the authors of this admirable treatise have not escaped scot free from the bias imparted by the prevailing trends. Professor Thompson is a psychologist, Dr. Fuller a geneticist. Both have carried out original researches on the genetics of animal behaviour; and this still seems to be their dominating field of interest. Indeed, when embarking on their task, they " considered omitting human genetics completely ": and, as they explain in their preface, they have deliberately avoided such old fashioned phrases as ' genetic *psychology* ' or ' *psychological* genetics '. Their firm resolve to describe their findings solely in terms of ' behaviour ' has resulted in several surprising omissions and a number of curious circumlocutions. For instance, to treat partial colour-blindness and similar conditions as primarily forms of ' behaviour ' is not merely awkward, but at times actually misleading. This mode of approach may be natural enough in studying animals; but it certainly detracts from the value which the authors' discussions might otherwise have for students of human beings. Nevertheless, in spite of its limitations, the work as a whole will prove a most welcome addition to every psychological library. J. Conway

Forces and Fields. By Mary B. Hesse. London: Nelson & Sons, 1961. Pp. x + 318. 35*s*.

As its subtitle indicates this volume is essentially a historical study of the notion of action at distance. The hypothetical models adopted by scientists to account for the various types of phenomena falling under this head are of two main kinds: those that accept the notion of a force acting on an object across an empty interval of space, and those that require continuous transmission by a medium or field. To a large extent the investigator's preference for one kind rather than the other is determined, not so much by his empirical observations as by his tacit preconceptions as to the rôle of theories and hypotheses in scientific explanation. Accordingly, Dr. Hesse begins her book with a preliminary disquisition on ' the logical status of theories ', and a detailed examination of ' operationism ', the ' hypothetico-deductive method ', and the use of scientific ' models '. Then in the body of the work she proceeds to trace the history of her subject from the early speculations of the ancient Greeks to those of the modern quantum physicist; and the book concludes with a chapter on the ' metaphysical framework of physics '.

The whole discussion, which is admirably documented, should be of the greatest interest to those psychologists who are concerned more especially with the methodological background of their general investigations. But there is a more particular reason why the paradox of action at a distance should appeal to the student of the mind. Francis Bacon in his classification of phenomena falling under this general head includes not only those in which the thing acted upon is a material substance or body, as in gravitation or magnetism, but also those in which it is a mind, as in the awareness of sounds, of warmth, and of visible objects, and in the communication by telepathy between friends or enemies or the influence of masterful personalities. Normal cognition, it will be noted, comes under this second category as well as paranormal. Sense-perception, as Burt has repeatedly argued, is essentially a mode of action at a distance. Dr. Hesse does not deal in any detail with normal cognitive processes; but she devotes several pages to the consideration of extrasensory perception.

Professor Broad, writing many years ago, put forward nine ' basic limiting principles ' to which he believed all Western scientists would subscribe, and which appear *prima facie*

to be contradicted by alleged ' psi-phenomena '. Of these methodological requirements, several, as Dr. Hesse points out, would no longer be endorsed by contemporary physicists; others raise difficult philosophical issues; and as to the remainder it would, she says, appear quite possible to devise a hypothetical mechanism which would allow us to accept such phenomena without violating any ' basic principles '. Nevertheless, she is inclined to agree that " some new explanatory concepts are needed "; and, although hitherto physical theories have proved somewhat unhelpful in suggesting working models to account for paranormal phenomena, it must still be to the conceptual systems of the physical sciences (she believes) that the psychologist will probably have to turn in the first instance for suggestive analogies. The concept of ' field ' is itself one of the most obvious. At bottom however the whole problem is not physical, but metaphysical, and, as she puts it in her closing words, " a society that is uninterested in metaphysics will have no physical science ".

T. R. Gregory

The Achieving Society. By David C. McClelland. New York: Von Nostrand, 1961. Pp. xvi + 512. $7.95.

Dr. McClelland, who is professor of psychology in the Department of Social Relations at Harvard University, is already well known in this country as the editor or co-author of works on motivation and kindred topics—notably *Talent and Society* and *Personality: The Achievement*. And the present volume is in many ways a sequel and a supplement to these two earlier publications. His problem is an ancient one. Why have certain societies at certain epochs produced so many great historical figures—statesmen, scientists, men of letters, men of business, while other societies have barely managed to survive? And why have the successful civilizations of the past so often ended in a decline and fall? The hypothesis he sets out to test is that the achievements of an ' achieving society ' are the consequences of its ' need for achievement '.

Three methods of investigation have been attempted based on (1) group measures of the ' need to achieve ' and other psychological variables, (2) individual measures of motives, values, interests, and the like, and (3) a more intensive study of the motives and behaviour of ' business entrepreneurs '. Free use is made of the analysis of stories, myths, literature generally, as well as of answers to systematic questionnaires. The results are compared with whatever data were available in regard to the economic development of the particular country in question during the particular period selected. The societies studied include those of ancient Greece, Spain in the Middle Ages, pre-Inca Peru, the United States, England under the Tudors, and during the industrial revolution and the Wesleyan revival, and (more briefly) Germany, Italy, Poland, Turkey, India, and Japan.

Dr. McClelland's final conclusions are highly instructive. The main ' sources ' of the ' need to achieve ' are in his view environmental, more particularly a temperate climate, certain cultural traditions, certain child-rearing practices, a middle class status, and a religious background of a quasi-protestant quasi-mystical type. Little attention is paid to genetic factors. Galton's eugenic theory is never mentioned. Dr. McClelland's chief example of ' biological explanation ' is a theory which he ascribes to Fanfani (an Italian politician of the thirties) who, as Dr. McClelland puts it, " decided that the shapes of their heads must be the determining factor ".

The book as a whole is a monument of industry. It owes much to the teams of advisers, assistants, and research associates who took part in many of the investigations or passed on their specialized knowledge of certain fields. Even readers who may be inclined to question the value of much of the quantitative work will nevertheless find the factual details and the deductions based on them as interesting as they are enlightening.

M. R. Johnson

The Logic of Personal Knowledge: Essays presented to Michael Polyani on his Seventieth Birthday: By PAUL IGNOTUS and other contributors. London: Routledge and Kegan Paul, 1961. Pp. xii + 248. 40*s*. net.

Michael Polyani is one of the most versatile of scientists. He was trained in medicine in Budapest, and later became a member of the Kaiser Wilhelm Institute of Physical Chemistry. Since coming to this country he has held two chairs at the University of Manchester, the first in Physical Chemistry and the second in Social Studies. His earliest book was on *Atomic Reactions*. To psychologists he is probably best known by his more recent works on *The Study of Man* and *Personal Knowledge*. The essays which compose this extremely readable Festschrift range over the whole scope of his multifarious interests. They start with technical papers on physics and chemistry, and proceed in turn to problems in the history of science, and the study of society, and conclude with certain semi-philosophical issues raised by the latest developments in chemical biology.

The contributions in these last three sections will have special appeal for psychological readers. The first is a short but fascinating paper by Arthur Koestler on ' Kepler and the Psychology of Discovery ': it traces the strange and circuitous path by which Kepler, as his inquiries progressed, passed stage by stage from the traditional type of cosmology that had survived for so long—a farrago of Platonic speculations, theological symbolisms, and animistic fantasies—to what is essentially the attitude of the modern scientist; and it describes, with apt quotations from Kepler's own voluminous writings, how he welded into a single dynamic whole the disconnected fields of sky geometry and terrestrial physics, and thus laid a firm foundation for the Newtonian universe. Mr. Koestler's paper is followed by a brief historical study by Miss C. V. Wedgwood on ' The Scientists and the English Civil War ', in her usual delightful and informative style, tracing a similar change of outlook that overtook this country during the twenty-seven years that intervened between Charles I's charter for the *Musaeum Minervae* and Charles II's charter for the Royal Society.

The next section contains a series of papers on problems from the borderland between social psychology and the related disciplines of economics, politics, and law. Here one of the most interesting contributions is that by Raymond Aron, comparing and contrasting the leading ideas of Polyani and Max Weber. The book concludes with three biological papers—on the logic of biology by Dr. Marjorie Grene, on the origin of life on earth and elsewhere by Professor Calvin, and an ingenious statistical inquiry by Professor Wigner, who attempts to estimate ' the probability of the existence of a self-producing unit '. He concludes his paper (and the book) with " a firm conviction that the existence of special ' biotonic laws ' stems from the overwhelming phenomenon of consciousness ".

CYRIL BURT

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The British Journal of Statistical Psychology is issued by the British Psychological Society. The subscription price per volume is 30*s.* net for Members of the Society and 30*s.* 6*d.* (post free) for non-members. The subscription price in the U.S.A. is $4.50 (post free). Members of the Society should send their subscriptions to THE SECRETARY, British Psychological Society, Tavistock House South, Tavistock Square, London, W.C.1 : non-members to Messrs. TAYLOR & FRANCIS, LTD., 18 Red Lion Court, Fleet Street, London, E.C.4. Until further notice the Journal will be issued in two parts each year, in May and November.

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Contributors are asked to send their papers in a form which is ready for submission to the printer : that is to say, the arrangement of the material should follow that observed in previous publications of this *Journal.* Each article should be headed either with a series of numbered headings in italic, corresponding with the cross-headings of the several sections, or with a brief abstract : (the latter procedure is preferred). Each section should have a short cross-heading, in capitals; and the whole should conclude with a summary embodying the main conclusions reached. Special attention should be paid to such details as correct spelling, grammar, capitalization, numbering, etc. (particularly in the case of tables and references). In preparing manuscripts and in correcting proofs authors should conform, so far as possible, with the ' Recommendations to Authors ' set out in *The Printing of Mathematics* by T. W. Chaundy, P. R. Barrett, and Charles Batey (Oxford University Press, 1954, ch. II, pp. 21–73), or to those given in the paper on ' Setting Mathematics ' (Arthur Phillips, *Monotype Recorder,* XL, iv, 1956).

Owing to the present conditions for printing, the cost of setting up tables and algebraic formulae which require hand composition is very much greater than the cost of machine composition. Contributors are therefore advised to arrange their material so that it can be printed as economically as possible. Often numerical results and algebraic formulae can, with a little simplification, be readily adapted for purposes of machine composition. Manuscripts involving many tables and numerous equations can as a rule only be accepted if the authors are willing to pay for the additional cost.

In future authors will receive two complete slip-proofs of their papers, but no page proof. They will be expected to pay the cost of all corrections for which they themselves are responsible.

THE BRITISH JOURNAL OF STATISTICAL PSYCHOLOGY

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THE BRITISH JOURNAL OF STATISTICAL PSYCHOLOGY

From the days of Galton onwards British psychology has been foremost in employing quantitative techniques for almost every type of problem, pure or applied. It was, however, only when the second world war was over that the British Psychological Society found itself in a position to add to the scientific periodicals which it published "a journal dealing primarily with the application of mathematical methods to psychological problems". The first issue appeared, under the title of *British Journal of Psychology, Statistical Section*, in 1947.

As the Editorial preface explained, the term "mathematical" was to be interpreted in the broadest possible sense. The articles printed deal with such widely different subjects as scaling methods, psychophysical methods, correlational and factorial procedures, the use of newer techniques such as analysis of variance and of covariance, the advantages and limitations of statistical techniques generally, and their relation to the wider background of psychology; and they are concerned not merely with the discussion of methods, but with reports of actual results.

Psychology by its very nature overlaps with a number of other highly specialized disciplines; and it is impossible to expect all its teachers to possess an up-to-date knowledge of the latest developments in so many relevant fields of science. Those who planned the journal therefore believed that a welcome innovation would be the inclusion of expository papers discussing the newer borderline topics as they arose; these consist either of full-scale book-reviews, dealing with the topic of the book as well as with the book itself, or of discussions of technical questions of current interest. Readers are regularly invited to suggest the subjects to be covered, and to submit, in the form of letters, criticisms or problems of their own. The outcome has been a special section for "Notes and Correspondence" which has proved a particularly popular feature.

Priority is given to British contributors, including those from Commonwealth countries. But papers likely to be of interest to readers of the journal are acceptable, no matter what may be their country of origin. In the past contributors have included writers of international repute from various countries —e.g. M. S. Bartlett, Raymond Cattell, J. Fraser Roberts, J. P. Guilford, Louis Guttman, Harold Hotelling, C. R. Rao, Godfrey Thomson, and Philip Vernon.

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